BOSTON UNIVERSITY
COLLEGE OF LIBERAL ARTS
LIBRARY

THE AMERICAN
JOURNAL OF PSYCHOLOGY
Founded in 1887 by G. STANLEY HALL

Vol. XLIII JULY, 1931 No. 3

FIVE BEHAVIORISMS

BY KATHERINE A. WILLIAMS, Radcliffe College

Both its proponents and opponents speak of behaviorism as if it were a school of psychology, or at least a well defined and more or less unitary system; yet a survey of the literature almost convinces one that there is no behaviorism, but only a multitude of behaviorisms. When a man proclaims himself a *Gestalt* psychologist we know quite well what he means; we know that he is in close agreement with Wertheimer, Koffka, and Köhler, even if we do not know what the views of these men may be. When a man proclaims that he is a behaviorist, however, we can only be sure that he is not in close agreement with any one else. There may be no perfect unanimity in the introspectionist, or the mentalist, camp, but that is understandable since the persons in it have been placed there by others outside the camp. They have not even chosen their own appellation but have had it presented to them. The behaviorist, on the contrary, hastens to seize the behavioristic banner and in the act of grasping it pauses to say that of course this does not mean allegiance to anybody or to anybody's ideas.

As a first step toward discovering what behaviorism really is, in so far as there is a behaviorism, and further what it ought ideally and logically to be, we shall here examine the views of five behaviorists; first, with respect to their positions on the more general and fundamental issues, such as the meaning of behaviorism, of consciousness, and of introspection; and, second, with respect to their accounts of specific psychological phenomena, such as sensation and

*Accepted for publication August 31, 1930. The italic numbers in the footnotes refer to the numbered references in the bibliography at the end of this article.

thought, which are crucial for them as objectivists, or with respect to topics, such as instinct and learning, which are regarded by them as central to their individual systems.

Watson

Watson was indisputably the first actively to espouse the cause of anything which might be called behaviorism. Watson's general position is most easily understood when it is remembered that his early work was done in the field of animal psychology. The affiliations of animal psychology are peculiarly evident so that it is not surprising that Watson asserts that psychology is a branch of biology. This is not an unusual assertion for a psychologist of any persuasion. It is obvious that psychology deals with living subjects and is, therefore, a 'life science.' But Watson means his pronouncement in a narrower sense. In the wider sense of 'life science,' sociology, economics, and history may be included. Watson, however, is thinking definitely of the 'natural sciences' of zoology, physiology, anatomy, and so on, and he is thinking of these as continuous in method and interest with physics.

The closest relation is sustained by behaviorism to physiology. The line cannot be drawn abruptly between these two, but behaviorism can, in a general way, be distinguished by the fact that it aims at the study of the behavior of the whole organism, while physiology studies the activity of parts. Watson does not stress the tracing of nerve-paths; this he leaves to the neurologist. He will be satisfied, as a psychologist, to trace connections between the response of the organism as a whole and the stimulus which led to this response.

It is the task of the behaviorist, then, to observe what stimulus calls forth what response in the human or animal organism so that, given the response, he can name the stimulus which produced it, or, given the stimulus, he can predict the response which it will produce. Life's most complicated acts are but combinations of these simple stimulus-response patterns of behavior. The terms "stimulus" and "response" must be interpreted broadly, so that *stimulus* includes, not only light-waves and so on, but also the most complex situations which can call out some sort of response, and *response* includes, not only simple reflexes like the knee-jerk, but also complex acts or adjustments such as getting food. The complex stimuli and responses can theoretically be analyzed into the simple physiological ones, but it is not always necessary for the behaviorist to do this.

What is the conception of consciousness involved in such a system? In his earlier writings Watson attempts to make this clear. He says that he will study only that which can be observed in the same way that his 'natural-science' colleagues observe their phenomena. They observe "without discussing or even being interested in the fact that there is an implied observer at every moment in science and that a thousand interesting metaphysical points lie behind an individual's ability to make observations."[1] This, he says, is a return to the naïve or non-reflective use of consciousness which regards it as the tool with which all scientists work, whereas introspective psychology, on the contrary, did not regard consciousness in this way, but made it, or its content, the object of observation, and regarded the method of observation as something distinctive, namely introspection.

But now Watson becomes impatient and dogmatic. He deprives consciousness of its modest place as a tool and denies it validity of any sort. It is neither provable as an entity nor useful as a concept. Watson testifies that he, for one, cannot taste or smell it and challenges any one to show him some. He will omit the study of consciousness from his science in the same way that the astronomer ignores astrology. Thus Watson's writings afford many vigorous assertions that there is no scientific use or evidence for consciousness, but it would be difficult to find an unambiguous declaration that there is no such thing.

The arguments against the method of introspection are that the results are not uniform and cannot be made so and that they do not tell us the important thing about a man, his ways of behaving. A limited use may be made of something which looks like introspection but which is called the verbal report and is asserted to be nothing but a response and to give information only of behavior.

The problems which are supposed to be particularly baffling to a behaviorist give Watson slight pause. As for sensations, he simply does not speak of such things. There are receptors, the stimuli which affect them, and responses. If a subject can make differential responses to variously colored lights, he discriminates colors; he has color-vision. If the subject cannot so respond, he cannot discriminate colors. In the case of human subjects the response may take the form of a verbal report, but this does not essentially alter the situation. After-images are effects of retinal stimulation which bring it

[1] *39*, 94.

about that a subject will act as if his retina were being stimulated although it has ceased to be so.

What is memory? Watson readily replies "*there is no faculty or process of memory*—there is only *learning* and *loss* of skill which comes from lack of practice."[2] If you want to find out whether a person remembers how to play golf, ask him to drive a ball. If he cannot, he does not remember. Even if he can tell you exactly how he used to do it, do not be misled into thinking that he remembers how to do it; he remembers only how to tell you that he did it—remembers only the verbal habits which he acquired when he was learning to drive a ball. Memory, then, is simply the ability to perform an action performed previously; if there is anything else it is merely the ability to repeat verbal activities connected with the actual performance. In any case, "all memories are at bottom motor memories."[3]

Thought is similarly a matter of bodily habits. It is an activity not overt, but "implicit," consisting of bodily sets, minute contractions of striped muscles in the speech mechanism and in other parts of the body, reactions of smooth muscles, and glandular secretions. Ordinarily the development of "manual" habits (that is, habits involving gross bodily movement) is paralleled by the development of verbal and visceral habits. Now when we rehearse in their customary connections these manual, verbal and visceral habits in the abbreviated "implicit" fashion we have a comparatively idle form of thought; when we use them in new combinations for the solution of difficulties, we have problem-solving thought. The verbal element is perhaps predominant, but Watson points out that many of our habits have never been paralleled by speech habits and yet may be used in thought. The unconscious of Freud consists of the individual's unverbalized habits. The mental image and the affective part of experience, two critical topics for an objectivist, are interpreted in terms of subvocal speech and visceral habits, respectively, while the meaning of an object for the organism is the total set of responses made by the organism.

Watson's own emphasis is likely to be placed on the denial of instincts and the correlated assertion of the importance of the conditioned reflex. The original equipment of the organism consists of numerous reflexes of a simple sort. These are modified—new stimuli

[2] *51*, 65. [3] *51*, 77.

are attached to the responses—and are combined to form complex modes of response, all upon the principle of the conditioned reflex. So important a part does this principle play that Watson says of the two men who have done most to formulate it, "Pawlow and Bechterew must be looked upon as furnishing the keystone to the arch of Behaviorism."[4]

On the strength of his work with infants Watson has concluded that there are no instincts, no inherited traits or abilities. There is inheritance of structure but there is no inheritance of function. Even the idea of innate differences in general intelligence is scouted. As a matter of fact, it is extremely difficult to interpret this insistence that function is not inherited. Surely no one thinks that there are inherited functions with no corresponding structures, nervous or otherwise; while Watson himself does not start the child out with mere structure and no function whatever. Even he supposes that there are a variety of reflexes, and a reflex is certainly a functional unit as truly as an instinct.

Indeed we read on to find that there are, according to Watson, actually rather complex patterns of behavior given innately—the emotions of fear, rage, and love. These are very like instincts, particularly as there is such a large component of gross behavior in them, as well as the visceral behavior which is rather more typical of emotions in Watson's system. But even these responses, though innate, are capable of great modification by conditioning, so that almost none of the adult behavior called by these names is innate.

Weiss

Weiss declares himself to be in substantial agreement with Watson, and he is probably correct in this, although it may be doubted whether Watson is in agreement with Weiss. That is, the views of Weiss, while not conflicting with those of Watson, have a very different tone, largely because Weiss's approach is much more ambitious philosophically.

To begin with, Weiss sees behaviorism, not as an outgrowth of animal psychology primarily, but as a solution of the body-mind problem. This solution consists in the denial of any entity or stuff, such as mind or consciousness, which is distinct from matter. Behaviorism recognizes exactly what physics recognizes and not a bit more. At present this reduces to electrons and protons in motion.

[4] *49*, 249.

As a matter of fact, the electron-proton hypothesis, although it receives frequent mention as a fundamental postulate, does not make much difference to Weiss's behaviorism, but the conception of motion as the only kind of activity goes deeper. He rejects purpose as a "fundamental psychological category," interpreting it, in so far as he uses it at all, in strictly mechanical terms, and proceeds upon the basis of a mechanistic determinism.

He defines behaviorism as "the science that studies the origin and development of those bodily movements (responses) of the individual which establish his status in the social organization of which he is a member."[5] This individual is a locus in the movement continuum which is the physical world. But the effect of hardihood conveyed by this announcement is somewhat injured when Weiss says, "Since, however, the locus of the *cosmic* movement continuum is in the sensori-motor system of the individual, its properties will depend upon the sensori-motor organization, and the properties of the movement continuum can thus only be described as functions of, or in terms of, human responses."[6] Once more, interpretation is difficult, but it would appear that if Weiss has saved a materialism for himself, it is a solipsistic and self-contradictory one that cannot be much comfort to him and probably will not be recognized by the physical sciences as their own position.

Consciousness, in this scheme, if it means anything at all (very often, says Weiss, it is only a term to cloak our ignorance), must refer to some sort of movement. Weiss is not entirely consistent in his statements of what the movements concerned in consciousness are. In his discussion of functional psychology he insists that consciousness must be a reaction, and apparently means an *effector process*. Again he seems to limit the application of the term to *certain types of effector process*, as when he says, "the behaviorist grants a status to consciousness as being a series of *speech habits* that one learns in a psychological laboratory."[7] In his discussion of physiological psychology, however, he takes quite another position. "For the behaviorist the mental series is regarded as only another *neural series*."[8]

Weiss's view of introspection is, at least superficially, similar to Watson's. He says, "Introspection is a form of behavior in which an individual *responds to his own responses*."[9] Watson, by a similar statement, means that introspection is a verbal response, and Weiss

[5] *64*, 133. [6] *64*, 52 f. [7] *64*, 240; italics are mine.
[8] *57*, 627; italics are mine. [9] *64*, 241.

at times says the same thing, for "introspection is merely the name for a group of speech reactions which conform to a particular terminology."[10] He does not, however, leave the theory in this comparatively intelligible and unambiguous form. In another place, he says, "from the standpoint of the behaviorist introspection is an *implicit* form of behavior in which the subject responds to his own responses."[11] The overt speech reaction, that is, is only the introspective *report*. The idea seems to be that every response serves as stimulus to certain further implicit and obscure responses which in turn lead to no overt response. When the instruction is given, "Report your mental state," however, these implicit responses (which constitute the 'introspection') are reënforced sufficiently to arouse a verbal ('introspective') report. The interpretation of this account is made especially uncertain by the fact that, whereas the report is apparently supposed to be a response to the implicit responses to the original response, it is supposed to be a *report* of the original response.

Confusion also appears in Weiss's treatment of more specific psychological phenomena such as the mental image and sensation. His account of the mental image is, on the whole, more intelligible than his account of sensation. He says, "The image [of a postage stamp] is rather the functioning of internal rather obscure motor patterns which at one time were released by a visual postage stamp stimulus but which now are no longer, in any sense, antecedents of the stamp-affixing reaction."[12] This indicates that the motor pattern *is* the image. Oftener, Weiss refers to neuro-motor or sensori-motor conditions of the organism as if *they* were the image. Upon occasion the neural conditions alone seem to afford the essentials of the image.

A sensation for Weiss is "a response in which it is possible to localize the stimulus and the receptors that release a conventionalized reaction."[13] The treatment of sensation is so involved with that of introspection that it is not plain whether the conventionalized reaction in the form, typically, of a verbal report or of some other discriminative reaction, *is* the sensation, or whether there is also a more internal response which stimulates the individual to make the conventionalized reaction. The following quotation, moreover, gives an account of sensation different from either of the possibilities just mentioned. "To have the *sensation of blueness* requires at least:

[10] *55*, 365. [11] *64*, 234; italics are mine. [12] *64*, 256. [13] *64*, 275.

(a) the spectral blue stimulus, (b) the specific retinal elements, (c) a specific sensori-motor organization which is the equivalent of the verbal sound-producing mechanism, *it is blue*. Now is not the *having of the sensation of blueness* merely the actual occurrence of these three conditions?"[14] But the end is not yet. Still another account is contained in the following: "That there seems to be a sense *quale* in addition to the sensori-motor conditions, is merely the fact that if my neuro-muscular processes are actually photographed at the time the landscape at which I am looking is photographed, even though the two photographs are not at all similar, it is still possible to work out a point for point correspondence or correlation between the two photographs and perhaps even classify the types of correlation into categories, the equivalent of what we call sense qualities."[15] Since Weiss speaks of a "sensation of blueness," there is no reason to think that by "sensation" he means the act of sensing and by "sense *quale*" he means the quality sensed, a distinction which might have made his theory more intelligible. We must, therefore, suppose that "sensation" and "sense *quale*" mean the same thing. The various possibilities seem to be, then, (1) that a sensation is an effector process, typically a speech or some conventionalized discriminative response for which the stimulus and the receptor can be "localized" (by the subject?); (2) that it is the occurrence together of the stimulus, the receptor, and the mechanisms of response; (3) that it is the fact of the correlations to be found between the neuromuscular processes and physical objects. Probably a blurred composite of the three would most nearly approach Weiss's actual idea.

In the rest of Weiss's doctrines there is nothing very distinctive except for his emphasis upon the "biosocial." We have seen that in his definition of behaviorism he takes the social relations of the individual to be very important—so important as to provide the criterion for the designation of the responses to be studied by behaviorism. He insists that the behaviorist must give an account of behavior in its "biosocial" aspects as well as in its "biophysical" aspects. The distinction between the biophysical and the biosocial refers to the fact that while a given bit of behavior can be regarded simply as a movement like any other, and can as such be studied biophysically, it can also be regarded as a stimulus to some other organism, or as itself a response to the behavior of some other organ-

[14] *64*, 272. [15] *64*, 279 f.

ism, and is thus to be studied biosocially. For instance, a word may be regarded as a response of certain muscles or it may be regarded as a response to another person. *Yes* and *Oui* are very different from the first point of view, the biophysical, but are identical from the second, the biosocial. Speech is the biosocial response *par excellence* and is most important for man's adjustment in his social environment. Thought, too, is to be treated primarily fron this viewpoint. In any instance of thinking there are discernible the situation which sets the problem, certain implicit responses, and the behavior which solves the problem. "From the standpoint of the behaviorist, thinking reduces itself to what we may designate as the problem-solution form of behavior."[16] A plurality of solutions to a given problem are likely to be biosocially identical although biophysically diverse; the implicit responses involved are likely to be even more various.

Lashley

Lashley has expressed his behavioristic doctrine in only two articles, but these two are so decisive that he must be ranked among the leading behaviorists. He differs from Watson and Weiss in the unequivocalness of his stand on the nature of consciousness. His position is that we must accept the account of the attributes of consciousness which the introspectionist gives us, but that in doing so we are not committed to acceptance of psychic entities or mind stuffs or anything that is not entirely accounted for by mechanics and chemistry. The implications of this become evident when he says of these mechanistic sciences, "They seek to keep their postulates as few and simple as possible; to avoid ascribing to the entities other attributes than those implied in a space-time-number system."[17]

Lashley denies that the distinction between physiology and psychology can be made on the supposition that physiology studies part reactions while psychology studies the reactions of the whole organism. According to him there is no more need for the concept of the whole organism in psychology than there is in physiology. Psychology simply *is* the "science of the physiology of reaction to stimulation."[18]

Consciousness is nothing more than reactions of bodily mechanisms. The concept of consciousness is not an important or distinctive one, for there is unbroken continuity between isolated reactions which are not conscious and the complex patterns of reaction which exhibit

[16] *4*, 323. [17] *9*, 246. [18] *9*, 271.

the attributes of consciousness, and these attributes can be described without residue in physical terms. In Lashley's view the concept of consciousness might be dispensed with entirely.

Meantime he summarizes his position in the following: "The conception of consciousness here advanced is, then, that of a complex integration and succession of bodily activities which are closely related to or involve the verbal and gestural mechanisms and hence most frequently come to social expression. The elements of content are the processes of reaction to stimulation and do not differ in essential mechanism from the spinal reflex of the decapitated animal to the most complex adaptive activity of man. The objects of awareness are the physical stimuli, but in every case they act by a process of summation in such a way that the logically discrete physical elements (physico-chemical processes) can not be reacted to separately and hence individually never become objects of awareness."[19] In a word, consciousness is patterns of bodily activity.

Now the question whether introspection can be employed in a study of such activity is a comparatively minor one of methodology. Like Watson and Weiss, Lashley believes that introspection is an organism's reactions to its previous reactions. Lashley's attitude is indulgent. He says, "there can be no valid objection by the behaviorist to the introspective method so long as no claim is made that the method reveals something besides bodily activity."[20] At the same time he thinks that other means are better. "The subjective view is a partial and distorted analysis. Behaviorism presents the possibility of a more nearly complete analysis of the same data."[21]

But, it will be asked, what is there in such physical reactions to account for the attribute of sensible *quality* which is so prominent a feature of the conscious content? Lashley examines the introspective account of quality, and finds that the only thing that can be said of quality is that it is not susceptible of analysis by introspection. Now Lashley thinks that there are physical systems which obviously have this attribute; that is, there are physical systems which are not divisible by other physical systems. More concretely, the organism, as a purely mechanical system, reacts in a given way to a given frequency of light-wave by a summation process—the waves are summated in the nervous system and the nervous impulses summate to produce the response. Another frequency

[19] *9*, 341. [20] *9*, 351 f. [21] *9*, 338.

produces another reaction. The machine (the organism) "cannot react to individual elements of the stimuli but only to the two complexes of stimuli as unanalyzably different. . . . This leaves over no unexplained residue, no psychic attribute."[22] To those who feel dissatisfaction with this account because it does not convey the proper idea of qualities in their sensory richness, Lashley makes the rejoinder that a scientific description does not have the function of arousing an experience of the thing described. If it did, the best possible description of the emotion of anger would be a blow in the face.

But having accounted for whatever qualitative core there may be in experience, Lashley points out that this core, or pure sensation, is never present alone but always with an interpretative motor set. When the interpretative motor set is predominant and when the qualitative aspect of experience is reduced, say, to the reaction to an entoptic stimulus, we have an image of some sort. When, however, the reaction to the stimulus is predominant, we have perception. There is an unbroken series between these two.

The traditional view of the self-transcendent nature of mental content is also treated by Lashley. He analyzes memory and meaning introspectively in the attempt to discover how they manifest this transcendence, and finds that they do not do it at all. A memory which is supposed in one way or another to transcend the here and now, is really nothing but a present image with a present feeling of pastness. The feeling of pastness is no indication of transcendence, for it may be present when the memory is not a true memory and refers to nothing actually in the past. The image has already been accounted for in behavioral terms; the feeling of pastness is likewise a bodily attitude; and so memory is reduced in its entirety to present bodily reactions. Meaning, too, is stripped of its special claims. Lashley adopts the context theory of meaning and maintains that "the only way in which an element of content may have meaning is by coëxisting with or by leading to another element of content, which is then the meaning of the first."[23] This is in Lashley's scheme, of course, reducible to a succession of responses of a behavioral sort.

Problem-solving or creative thinking does not require the postulation of non-physical entities. It is reducible to a series of positive responses followed by incompatible negative responses, with the

[22] *9*, 331 f. [23] *9*, 259.

inevitable blockings and startings over again, until a train of responses is hit upon which leads to no blocks, whereupon relaxation occurs and the problem is solved.

Lashley's experimental work has been concerned almost entirely with studies of the function of the brain, particularly in learning. Although he started with the prevailing belief in the reflex-arc type of neurology and the conditioned-reflex type of theory of learning, the results of his research have led him to doubt these theories. While it is too soon to see precisely what he will substitute for them, it is not too soon to see that it will be something which will differentiate him sharply from the Watsonian school with its one explanatory principle of the conditioned reflex. The following quotation gives an idea of the sort of thing that we may expect from Lashley. "I believe that there is ample evidence to show that the units of cerebral function are not single reactions, or conditioned reflexes as we have used the term in America, but are modes of organization. The cortex seems to provide a sort of generalized framework to which single reactions conform spontaneously, as words fall into the grammatical form of a language."[24] Various neural processes are not isolated units, but are interdependent so that injury to one is reflected in all others. Lashley further says, "this unity of action seems to be more deeply rooted than even the structural organization."[25] This same relative independence of "particular units of structure" and dependence upon "the relationship among the parts"[26] is exhibited by the phenomena of growth. In that direction the explanation of them is to be sought.

Hunter

The evolution of Hunter as a behaviorist is more obvious than that of the others here examined. In the 1923 edition of his *General Psychology* he writes, "It is possible to divide the subject-matter of psychology into two significant classes: the facts of consciousness and the facts of behavior."[27] In 1928 he writes in *Human Behavior*, "The subject-matter of anthroponomy is behavior."[28] The *General Psychology* makes full use of the material and concepts of introspective psychology, differing from a standard text-book only in its greater emphasis upon the facts of behavior. The 1928 volume is the same book painstakingly revised to avoid all mention of introspective concepts. The material of introspective psychology is

[24] *11*, 17 f. [25] *11*, 19. [26] *11*, 20. [27] *1*, 4. [28] *7*, 2.

retained in very large part but the discussions of the first editions are translated, by hook or by crook, into behavioristic terms. The details of this interesting *tour de force* are less significant for our purpose, however, than the author's expression of the principles which motivate it.

It is odd to find a man, most of whose experimental work has been done with animals, naming his science "anthroponomy" and defining it as follows: "Anthroponomy is the science of the laws which govern human action—the science of human nature."[29] The study of animals is designated "phylogenetic anthroponomy" and has value as a comparative study of animal and man. Only incidentally has it an interest of its own.

Anthroponomy is marked off from biology by no sharp line but only by the traditional difference in subject-matter of the sciences of biology and psychology. (Psychology is the forerunner of modern anthroponomy just as astrology is the forerunner of modern astronomy.) If there is ground for a distinction, it is to be found in the fact that anthroponomy is more interested in studying the adjustment of the individual to his social and non-social environments than is biology.

Hunter admits that a flat denial that consciousness exists is likely to prove unsatisfactory and offers his view as an escape from this behavioristic dogma as well as from the prevalent unsatisfactory theories of consciousness. His account is this. Where SP stands for sensory process and LR for language response, *"The irreversible relationship SP-LR is the phenomenon termed 'consciousness'."*[30] By "*irreversible* relationship" he means that we have the phenomenon of consciousness not when the language response arouses the sensory process but only when the sensory process arouses the language response. What "relationship" means it is somewhat difficult to know. The temptation is to interpret the phrase "SP-LR relationship" as "language response aroused by a sensory process." Hunter, himself, occasionally slips into this mode of expression as when he says, "I have attempted to show that 'consciousness' is a term applied by the subjectivist to a phenomenon whose typical form is that of a language response aroused by sensory stimulation."[31] Most of the time, however, he insists that consciousness is not the response but literally the relationship. If the response itself is to be conscious, he says that

[29] *5*, 286. [30] *2*, 11. [31] *3*, 478.

it must arouse another language response, it must be the beginning of the situation.

The next difficult term in the definition is "sensory process," or "SP." Sometimes Hunter uses SP interchangeably with "stimulus," sometimes with "peripherally initiated neural processes," sometimes with "stimulation," and sometimes with "sensation." It probably most nearly represents his meaning to say that the sensory process, or SP, is the whole chain of events initiating and involving receptor processes. If we must select a single term, "stimulation" probably comes nearest to Hunter's meaning.

By "language response," or LR, Hunter does not refer exclusively to verbal responses. Almost any response may serve as a language response provided that it is symbolic and can be reinstated by the organism itself to control further responses. (He also adds that it must have a social significance—a criterion which excludes animals.) A symbolic process is a substitute response of the conditioned-reflex type in which the associative connections with the original stimulus have not dropped out, as happens when the response has become automatic, but are still present.

This account is an account of consciousness itself; it is not simply an account of the situation in which consciousness occurs, consciousness itself being something over and above these objective features of the situation. The irreversible relationship *is* consciousness and there is nothing else to be discovered beyond the establishment of the relationship. If consciousness seems to be present without this language response or before the language response occurs, it is because of the presence of implicit and unrecognized language responses or of neural processes which act as their equivalent.

Hunter's discussion of the "subject's report" treats of the problems of introspection. According to Hunter, any bit of behavior may be a report of the conditions which initiated it and of those upon which it depends; that is, it is evidence for these conditions. Thus a sneeze, for instance, may be a report of irritated nasal passages. Now a language response may likewise be a report. It *is* a report to the experimenter, however, only when it is the stimulus for a language response of the experimenter's own, and similarly it is a report to the subject himself only when it is a part of an SP-LR process of his own; that is, a report must be conscious. Anyhow, "it is always to be remembered that the verbal response does not *describe*

the stimulus any more than does the non-verbal response."[32] It is simply a symptom and what it is symptomatic of is often a matter of doubt. The verbal report, therefore, must be used cautiously. Introspective psychology, says Hunter, attempted to limit itself to the study of the SP-LR processes, but this is "an unwise and undesirable effort."[33]

Hunter's account of consciousness so far has proved to be far from illuminating but it is supplemented by certain metaphysical considerations which, for one reader at least, have introduced still more confusion. Hunter argues that the behaviorist is not open to the charge of ignoring a part of the world, the psychic part, because no one has shown that the environment has psychic or mental characteristics. His position evidently is that the so-called psychic, or mental, includes simply what has been called the content of consciousness, while the content of consciousness in turn is, when properly understood, simply the environment, which ought probably to be called mental. He says that "books, pains, hungers, tastes, colors, and melodies" are "constituents of the outer and inner environments as viewed by common sense."[34] He proceeds further to justify the behaviorist by giving behavioristic accounts of the nature of the environment; but he makes now the startling assertion that in his own view the environment is the irreversible SP-LR relationship!

The situation seems to be that Hunter has two conceptions of consciousness which are incompatible, but that he has boldly accepted the consequences of each. First he says that consciousness is the irreversible SP-LR relationship; next that consciousness is the environment; and then goes on to the inevitable inference that the environment is the irreversible SP-LR relationship, that is, the relationship between sensory process and language response. After this outrage to common sense he yet plaintively remarks that the behaviorist is often criticized unjustly because his critic has forgotten that "the behaviorist, like the physicist, accepts a commonsense view of the environment as the milieu for his experimentation."[35]

Of Hunter's specific psychological doctrines, his account of sensation does not require special mention, since, with the modifications implied by his general theory of consciousness, it is similar to Watson's. It will be well, however, to summarize his account of thought. Thought is distinguished from trial-and-error behavior by the fact

[32] 7, 220. [33] 2, 29. [34] 8, 287. [35] 8, 296.

that in true thought the problem is formulated in language which helps to determine subsequent behavior. There is thus a language element and a use of substitute behavior which is not to be observed in mere trial-and-error behavior. The formulation of the problem takes the form of instructions either given by the subject himself or given from without and accepted by him. Here purpose is to be found. "The control of behavior sequences by the instruction stimulus gives to behavior that aspect called purpose."[36] Again, "there is no question that goals are attained by most responses, but the only way in which a goal can influence preceding behavior is through the facilitating and inhibiting influence of instructions which in their turn are reinstated language responses."[37] It is worth remarking that Hunter has restricted both thought and purposive behavior to human beings. The precursors of language are found in the delayed responses of animals but true language is probably found only in man, and hence thought and purposive behavior are also restricted to him.

Tolman

Tolman has the most complete and closely knit psychological system, as well as the most constructive and original. Like Watson, he has for background an interest in animal psychology, but here the resemblance ceases. Watson supports the view that psychology is to be distinguished from physiology by the fact that the former studies the reactions of the whole organism; and Tolman, too, makes this assertion, but he evidently means it more whole-heartedly than Watson. A large part of Watson's psychology consists, after all, in showing how the behavior of the whole organism is to be understood only in terms of physiological reflexes Tolman, however, does not propose to be much concerned with 'muscle twitches and glandular secretions.' He confesses that the behaviorist often has very little knowledge of such affairs. Instead, he will take the larger, non-physiological, functional units of behavior as his data, for it is these, he believes, which provide psychology with its proper subject-matter.

Tolman's attitude toward consciousness shows some uncomfortableness. He admits the existence of what he calls "raw feels," that is, sense *qualia*, but he is distinctly unwilling to quarrel with any one who declines to do likewise. He sometimes refers to these

[36] 7, 336. [37] 7, 336.

inconvenient entities as "private mental 'feels,' if there are private mental 'feels'."[38] At any rate he is firm in the conviction that these feels cannot be dealt with introspectively. The introspectionist himself in his analysis of qualities can ultimately only point to similars. He is reduced, that is, to behavior which indicates only the relations of these *qualia*.

"Consciousness," then, is not used by Tolman to refer to these feels. He gives it a behavioral meaning; that is, consciousness is to be found in the moment of "re-gestalting," or learning. According to Tolman, behavior expresses certain cognitions. Take, for instance, a rat which learns to discriminate between the black and the white sides of a discrimination-box. His constant choice of the white side exhibits a cognition of the difference between black and white. But this is not necessarily conscious; it may be quite habitual and automatic. However, the moment at which the rat begins to react to the two sides, not as alike but as different, is the conscious moment. "Acts which imply more cognitive differentiation may be just as automatic as ones which imply less cognitive differentiation. It is only the switch-over when it occurs in a given moment of stimulation that defines consciousness."[39] Tolman's explanation of the switch-over is among the least satisfactory of his achievements from the strictly objective point of view. The switch-over takes place by means of a "representation" of the "probable stimulus-results to be expected from the act."[40] The representation is not a mental image, of course, but is a "behavior-adjustment." That is, the animal, instead of making an overt and complete response to the white and black sides of the discrimination-box, makes only an adjustment for this response, an implicit response, a sort of feint. "Stimulus-results" is not an altogether clear expression. It might mean the stimuli which would result from the carrying out of the response. If so, it is hard to see what these could be unless they were mental images. "Stimulus-results" might, however, mean behavior such as would result from the stimuli with which the carrying out of the behavior would bring the organism into contact. This might be thought of as occurring without the mediation of the actual stimulus upon some such principle as that of the conditioned reflex. Although some of Tolman's earlier statements lead to the view that the first

[38] *23*, 360. [39] *24*, 435. [40] *24*, 436.

meaning is his real one,[41] his later analyses lend themselves to the more consistently objective interpretation.

According to Tolman's account of sensation two elements are to be distinguished, namely the *quale* and the *term character*. Tolman uses "*quale*" to refer to the "immediate raw feel" of, say, brownness, while by the "term character" of brownness he means its relations, especially similarity and difference, with other qualities. Now only the term character of a quality can be communicated by one person to another. No matter how persistent I may be in my attempt to describe my raw feel of brownness to you, I cannot do more than indicate to you what the term character of brownness is. This I do by means of my behavior in discriminating between brown and other colors, in sorting brown from among other colors, in arranging browns in the order of their likeness to reds, yellows, oranges, and grays, and so on. "That is, the world of stimuli presents a certain system of similarities and differences for each organism. In the case of a fellow human this system may be the same as it is for me. In the case of a visitor from Mars it may or may not be the same. But in either case it is only this system of similarities and differences which I learn about and never the immediately experienced *qualia* as such."[42] From this Tolman concludes that sensation-qualities, in so far as they can be known by psychology, reduce to mere term characters. In an instance of memory of a sense-quality, the term character may be accurately indicated by behavior although the *quale* is no longer present, and may even be replaced by another *quale*.

Although Tolman tends to make increasing use of the terms and schemata of *Gestalt* psychology, he maintains throughout his own doctrine that the central concept necessary for the understanding of behavior is that of purpose. "Purpose" is regarded as a perfectly objective concept; purpose is objectively definable and purposes are objectively observable. A purpose is the "persistence-until" aspect of behavior. A cat struggles in a puzzle-box *until* it is released; a bird flies about collecting strings *until* a nest has somehow been constructed. Tolman does not mean that this 'persistence-until' is a behavioral correlate of a mental purpose, or that it marks out for us the kind of behavior from which we may infer an inner purpose. He means that this 'persistence-until' in behavior *is* purpose, that when we see an instance of 'persisting-until' in behavior we have

[41] *13*, 229. [42] *15*, 144.

before us an instance of purpose. The two bits of behavior mentioned above, for instance, cannot be understood, they cannot even be identified, without reference to the ends achieved. "In each case the trials (and errors) *keep on until* some particular and objectively discoverable end-object or situation is got to or from."[43] "These purposes to get to or from are part of the very descriptive texture of the act."[44]

Fundamentally purposes are persistences to or from physiological states, or rather they are persistences from one physiological state to another. Tolman includes in the list of primary drives or purposes: food-hunger, sex-hunger, shelter-demands, excretion-demands, fatigue-demands, and aesthetic demands, all of which are appetites or drives toward quiescences; and fear and pugnacity which are aversions or drives from disturbances. He concludes, "the important point for this discussion, however, is merely the doctrine that all behavior-purposes do thus reduce ultimately to drives to or from final physiological states, and that all other objects or situations are, in the last analysis, got to or from only as routes or means for getting to or from these bodily states."[45]

Cognition, instinct, and learning all fall into this scheme. In behavior there are recognizable hierarchies. The hungry cat in the puzzle-box struggles until it gets out and eats the food. Now there are observable the superordinate act of satisfying the hunger, and the successively subordinate acts of getting to the food, of getting outside the box, of pulling the latch, and so on. Each of these acts expresses certain cognitions with respect to the condition initiating the act, to the goal of the act, and to the relations between the initiating condition and the goal. That is, the struggling which finally releases the cat will not take place unless the box actually presents certain features. The struggling, therefore, postulates these features. The food is not eaten unless it satisfies hunger. The eating of the food postulates that food has the property of satisfying hunger. The struggling will not take place if the food is within the box. The struggling, therefore postulates a certain relation between the food and the interior of the box.

The subordinate acts are bound to the superordinate acts by the fact that they are involved in the postulations made by the larger act. Now when the organism is innately equipped with certain

[43] *23*, 355. [44] *23*, 355. [45] *23*, 357.

modes of response and their "immanent" postulations, we have a case of instinct. The instinct is loosely organized to the extent that the subordinate acts are variable. Learning consists in the improvement of just such postulations. This is accomplished by a "re-gestalting" of the elements of the situation. In the case of trial-and-error learning the re-gestalting requires actual trying out of the various responses and retention of the elements discovered. Insight learning is a re-gestalting of elements already present perceptually or memorially. But even memory does not take us outside the objective. "The animal's going-to-or-from an object, not immediately present, does, to be sure, imply that his behavior is then and there a function of a now absent object. But this temporo-functional dependence of behavior is a purely objective fact and all that need be meant by memory."[46].

There is some uncertainty to be found in Tolman's treatment of most of his central concepts. There is no doubt that they are all defined by reference to objective behavior although to just what sort of behavior is not always clear. Tolman uses all of the following expressions, which surely do not mean precisely the same thing. Purpose is a behavior showing 'persistence-until;' purpose is the 'persistence-until' aspect of behavior; purpose is a descriptive feature immanent in the character of behavior; purposes are persistences to or from. Again he says that purposes are drives, that purposes provide drives, that appetites and aversions are two kinds of drive, that they provide drives, that they provide purposes. The situation with respect to what "postulation" means is not much clearer. At one time or another he says: the response postulates; the response expresses a postulation; it exhibits a postulation; it is a postulation; postulation is immanent in behavior; postulation is immanent in the fact that behavior persists only if such and such is the case; postulation resides in the dependence of the persistence aspect of behavior upon there being certain actual characters in the end-object; the animal postulates.

There is a tendency in Tolman's later writings to interpret "purpose," "postulation," and "cognition," as immanent functional determinants of behavior which the psychologist infers from the behavior itself. These determinants are presumably patterns (*Gestalten*?) that are no more transcendent of physical reality than the configurations inferred by the physicist.

[46] *21*, 40.

Recapitulation

The points on which behaviorists agree are few. The really defining character of a 'behaviorist' seems to reduce to adherence to the doctrine that scientific psychology can profitably study only such facts as are open to objective methods of observation. As to whether there are any other facts than these objectively observable ones, some say definitely that there are not, while some are inclined to be evasive. Lashley and Weiss deny the existence of any entities not physical in their nature, and therefore of any entities not capable theoretically at least, of being objectively observed. Watson and Hunter virtually deny that mental entities exist, while Tolman virtually admits it.

The ambiguity of the English language coöperates with lack of philosophical sophistication to prevent very often the making of a clear distinction between the two uses of the word 'consciousness' which are implied by two such phrases as 'conscious quality' and 'conscious organism.' It may be that this distinction, the behavioral analogue of the old epistemological one of content and act, may not be ultimately valid, and some of our behaviorists may intend to identify the content and the organic process. This would be rather a remarkable metaphysical hypothesis, however, and is impossible of clear expression without some sort of prior recognition of the distinction indicated. If we do our best to impose this distinction where it is not clearly made, we find that Watson pretty well avoids the question of the locus of what is usually called the content of consciousness. Lashley and Weiss, and sometimes Hunter, definitely locate this content within the organism, while Hunter sometimes flirts with a hypothesis very similar to the neo-realistic one that it is actually a part of the physical environment. Tolman regards all this as a metaphysical question, and as a decorous psychologist declines to meddle with it.

But our behaviorists do not agree any better on how the process of consciousness in the organism is to be behaviorally interpreted. Although not all of them are clear in their own minds on this issue, on the whole, Watson, Weiss, Lashley, and Hunter seem to agree that speech reactions are of central importance in the matter. Tolman, on the other hand, sees in the moment of learning the essence of consciousness.

There is agreement that objective methods of observation are the only permissible ones, but the verbal report, sometimes called introspection, is admitted by all these psychologists in so far as it

pretends to be no more than a response to a response and to do no more than give information about behavior. True introspection, as a peculiar method of observing conscious states, is banned by all.

Our five agree, of course, that behavior is the proper subject-matter of behavioristic study, but *behavior* is so broad a term that further specifying is needed, and here our five part company. Watson and Tolman regard the reactions of the whole organism as the proper objects of study, and hold that this distinguishes psychology from physiology. In practice Watson is, perhaps, farther from Tolman than he is from Lashley, who argues that psychology is simply that part of physiology which studies the responses of organisms to stimulation. Weiss makes adjustment to a social environment the criterion of the behavior to be selected for examination by psychology, and Hunter inclines to do the same. This places them closer to Watson and to Tolman than to Lashley.

There is no agreement on the general attributes of behavior. Weiss and Watson, particularly Watson, insist that behavior is always mechanical, a mere complication of mechanical reflexes. Tolman sees it as through and through purposive. Lashley, in spite of his view that psychology cannot be distinguished in attitude from physiology, is nearer Tolman on this point than to Watson and Weiss, for his physiology is not of the rigid reflex type but of the purposive type. Hunter limits purposive behavior to human problem-solving thought.

There is only one point of specific psychological doctrine upon which all of our behaviorists agree. This happy unanimity is found with respect to one of the most difficult problems with which they are confronted, that of sensation, but it does not go very deep. They merely agree that the essence of sensation is to be sought in the discriminative aspect of response. For the rest, there is discord. Watson, Lashley, and Tolman see thought as a matter of implicit responses primarily, while Weiss and Hunter lay the emphasis upon the actual problem-solving behavior. Watson denies instincts, while Tolman and Hunter admit them. For Watson learning is the acquisition of conditioned reflexes; for Tolman and Lashley it is something much less mechanical

At present, behaviorism, if it is to be defined as the body of opinion upon which leading behaviorists agree, is scarcely more than a methodology. However, the diversities (even within the individual systems) promise much for the future. They not only indicate liveliness of interest but insure a thorough canvass of possibilities.

Bibliography

1. W. S. Hunter, *General Psychology*, rev. ed., 1923.
2. ——, The problem of consciousness, *Psychol. Rev.*, 31, 1924, 1-31.
3. ——, The symbolic process, *Psychol. Rev.*, 31, 1924, 478-497.
4. ——, The subject's report, *Psychol. Rev.*, 32, 1925, 153-170.
5. ——, General anthroponomy and its systematic problems, *Amer. J. Psychol.*, 36, 1925, 286-302.
6. ——, Psychology and anthroponomy, *Psychologies of 1925*, 1926, 83-107.
7. ——, *Human Behavior*, 1928.
8. ——, Anthroponomy and psychology, *Psychologies of 1930*, 1930, 281-300.
9. K. S. Lashley, The behavioristic interpretation of consciousness, *Psychol. Rev.*, 30, 1923, 237-272, 329-353.
10. ——, *Brain Mechanisms and Intelligence*, 1929.
11. ——, Basic neural mechanisms in behavior, *Psychol. Rev.*, 37, 1930, 1-24.
12. E. C. Tolman, Nerve process and cognition, *Psychol. Rev.*, 25, 1918, 423-442.
13. ——, Instinct a d purpose, *Psychol Rev.*, 27, 1920, 217-234.
14. ——, A new formula for behaviorism, *Psychol. Rev.*, 29, 1922, 44-53.
15. ——, Concerning the sensation quality—a behavioristic account, *Psychol. Rev.*, 29, 1922, 140-145.
16. ——, Can instincts be given up in psychology?, *J. Abn. and Soc. Psychol.*, 17, 1922, 139-152.
17. ——, Mr. Mursell's concept of sensation, *J. Philos.*, 20, 1923, 271-272.
18. ——, A behavioristic account of the emotions, *Psychol. Rev.*, 30, 1923, 217-227.
19. ——, The nature of instinct, *Psychol. Bull.*, 20, 1923, 200-218.
20. ——, Purpose and cognition: the determiners of animal learning, *Psychol. Rev.*, 32, 1925, 285-298.
21. ——, Behaviorism and purpose, *J. Philos.*, 22, 1925, 36-41.
22. ——, The nature of the fundamental drives, *J. Abn. and Soc. Psychol.*, 20, 1926, 349-358.
23. ——, A behavioristic theory of ideas, *Psychol. Rev.*, 33, 1926, 352-369.
24. ——, A behaviorist's definition of consciousness, *Psychol. Rev.*, 34, 1927, 433-439.
25. ——, Habit formation and higher mental processes in animals, *Psychol. Bull.*, 24, 1927, 1-35.
26. ——, Habit formation and higher mental processes in animals, *Psychol. Bull.*, 25, 1928, 24-53.
27. ——, Purposive behavior, *Psychol Rev.*, 35, 1928, 524-530.
28. J. B. Watson, Psychology as the behaviorist views it, *Psychol. Rev.*, 20, 1913, 158-177.
29. ——, Image and affection in behavior, *J. Philos.*, 10, 1913, 421-428.
30. ——, *Behavior, an Introduction to Comparative Psychology*, 1914.
31. ——, The place of the conditioned reflex in psychology, *Psychol. Rev.*, 23, 1916, 89-117.
32. ——, The psychology of wish fulfillment, *Scient. Mo.*, 3, 1916, 479-487.
33. ——, Behavior and the concept of mental disease, *J. Philos.*, 13, 1916, 589-596.
34. ——, Does Holt follow Freud?. *J. Philos.*, 14, 1917, 85-93.
35. ——, An attempted formulation of the scope of behavior psychology, *Psychol. Rev.*, 24, 1917, 329-352.
36. —— and J. J. B. Morgan, Emotional reactions and psychological experimentation, *Amer. J. Psychol.*, 28, 1917, 163-174.
37. ——, A schematic outline of the emotions, *Psychol. Rev.*, 26, 1919, 165-196.
38. —— and R. Raynor, Conditioned emotional reactions, *J. Exper. Psychol.*, 3, 1920, 1-14.
39. ——, Is thinking merely the action of language mechanisms?. *Brit. J. Psychol.*, 11, 1920, 87-104.

40. ——, Behaviorism; the modern note in psychology, *Psyche*, 5, 1924, 3-12.
41. ——, The place of kinesthetic, visceral and laryngeal organizations in thinking, *Psychol. Rev.*, 31, 1924, 339-348.
42. ——, The unverbalized in human behavior, *Psychol. Rev.*, 31, 1924, 273-280.
43. ——, *Behaviorism*, 1924.
44. ——, Experimental studies on the growth of the emotions, *Psychologies of 1925*, 1926, 37-58.
45. ——, Recent experiments on how we lose and change our emotional equipment, *Psychologies of 1925*, 1926, 59-82.
46. ——, What the nursery has to say about instincts, *Psychologies of 1925*, 1926, 1-36.
47. ——, Behaviorism: a psychology based on reflexes, *Arch. Neur. and Psychiat.*, 15, 1926, 185-204.
48. ——, Behaviorism: a psychology based on reflex-action, *J. Philos. Stud.*, 1, 1926, 454-466.
49. ——, Origin and growth of behaviorism, *Arch. f. syst. Philos.*, 30, 1927, 247-262.
50. —— and R. R. Watson, *Psychological Care of the Infant and Child*, 1928.
51. ——, *The Ways of Behaviorism*, 1928.
52. ——, *Psychology from the Standpoint of a Behaviorist*, 3rd rev. ed., 1929.
53. ——, Behaviourism, *Enc. Brit.*, 14th ed., 1929.
54. A. P. Weiss, Relation between structural and behavior psychology, *Psychol. Rev.*, 24, 1917, 301-317.
55. ——, Relation between functional psychology and behavior psychology, *Psychol. Rev.*, 24, 1917, 353-368.
56. ——, Conscious behavior, *J. Philos.*, 15, 1918, 631-641.
57. ——, The relation between physiological psychology and behavior psychology, *J. Philos.*, 16, 1919, 626-635.
58. ——, The mind and the man-within, *Psychol. Rev.*, 26, 1919, 327-334.
59. ——, Behavior and the central nervous system, *Psychol. Rev.*, 29, 1922, 329-343.
60. ——, The stimulus error, *J. Exper. Psychol.*, 5, 1922, 223-226.
61. ——, Behavior and behaviorism, *Psychol. Rev.*, 31, 1924, 118-149.
62. ——, One set of postulates for a behavioristic psychology, *Psychol. Rev.*, 32, 1925, 83-87.
63. ——, Discussion: purposive striving as a fundamental category of psychology, *Psychol Rev.*, 32, 1925, 171-177.
64. ——, *A Theoretical Basis of Human Behavior*, 1925.
65. ——, A set of postulates for social psychology, *J. Abn. and Soc. Psychol.*, 21, 1926, 203-211.
66. ——, Dr. Davies on mechanism and teleology in behavior, *Amer. J. Psychol*, 37, 1926, 450-459.
67. ——, Mr. Gundlach on "some difficulties with Weiss's behavioristic postulates," *Amer. J. Psychol.*, 38, 1927, 669.
68. ——, Behaviorism and ethics, *J. Abn. and Soc. Psychol.*, 22, 1928, 388-397.
69. ——, Emotions as forms of behavior, *Wittenberg Symposium*, 1928, 170-192.

INSTRUCTIONS AND THE PSYCHOPHYSICAL LIMEN

By SAMUEL W. FERNBERGER, University of Pennsylvania

The problem of the use of equality judgments in psychophysical procedures has been unsettled since the time of Fechner. A review of the historical background of this problem has been recently published.[1] It will be remembered that, for the method of constant stimulus differences, the relative number of equality judgments is of primary importance, inasmuch as the area bounded by the psychometric function for the equality judgments and the abscissa is equal to the rectangle with the interval of uncertainty as base and with unity as height. Thus the number of equality judgments determines the size of the interval of uncertainty. Urban[2] has recently said: "Das ganze Verfahren wird sinnlos, wenn die Gleichheitsurteile durch die der Vp. gegebene Instruktion ausgeschaltet werden, da ja kein Versuch beendet werden kann, bevor die Vp. die Gleichheit zwischen Haupt- und Vergleichsreiz wahrnimmt."

One difficulty with the equality judgments is the fact that they are not only affected by certain objective conditions but by subjective influences as well. Frank Angell has shown that some observers give more equality judgments than others and he suggested that "this difference corresponds to the differences between deliberate and impulsive temperaments."[3] A number of investigators have shown that the equality judgment is affected by the attitude of the observer and by the instructions.[4] It has also been shown introspectively that the conscious attributive pattern for the equality judgment is different from that of either of the difference judgments, involving doubt and verification.[5]

*Accepted for publication August 7, 1930.

[1] S. W. Fernberger, The use of equality judgments in psychophysical procedures, *Psychol. Rev.*, 37, 1930, 107-112.

[2] F. M. Urban, Die Methode des durchschnittlichen Fehlers, *Arch. f. d. ges. Psychol.*, 74, 1930, 159.

[3] F. Angell, On judgments of "like," this JOURNAL, 18, 1907, 253-260.

[4] Fernberger, The effect of the attitude of the subject upon the measure of sensitivity, this JOURNAL, 25, 1914, 538-543; S. S. George, Attitude in relation to the psychophysical judgment, *ibid.*, 28, 1917, 1-37; F. H. Reiter, *The Influence of Instruction on the Formation of Judgments in Lifted Weight Experiments* (Univ. Pennsylvania thesis), Philadelphia, 1917.

[5] Fernberger, An introspective analysis of the process of comparing, *Psychol. Monog.*, 26, 1919, (no. 6), 1-161.

The following experiment is an effort to determine more exactly the effect of instructions on the judgments of the different categories.

Experimental Procedure

Lifted-weights were used throughout the present experiment. The objective conditions were those carefully described elsewhere.[6] Two series of 7 pairs of weights were used, with the standard weight of 100 grm. and the comparison weights in 4 grm. intervals from 84 to 108 grm. The time error was in the first order, with the standard weight lifted first. The order of the weights was changed several times during the experiment to prevent the learning of the series.

The following persons kindly acted as *Os* in what proved to be a lengthy series of judgments: Agnes Kamper (*K*), William Van Buskirk (*B*), Leon Arons (*A*), all graduates students, and Professor H. Sherman Oberly (*Ob*). *K* and *B* were entirely unpracticed in the formation of lifted-weight judgments at the beginning of the experiment. Their judgments were recorded from the very first as soon as their hand movements had become automatic as the result of training in the lifting of a single weight. *A* and *Ob* were both practiced in the lifted-weight procedure as the result of previous experimentation.

The Instructions

Four types of instructions were employed. In each of the four cases the instructions were identical, except for the prescribing and the definition of the categories of judgment. Each instruction began as follows.

I am going to ask you to judge the relative intensity of a number of pairs of weights. No information with regard to the actual relative weight of any of the pairs will be given you. You are asked to judge in terms of the following series of categories, defined as follows.

Here followed the definition of categories which will be found for the four cases below and then the instructions concluded as follows:

You will report the judgment immediately after the lifting of each second weight in terms of one of the categories prescribed. In case your judgment cannot readily fit into one of the prescribed categories, you will stop the experiment immediately and indicate the cause at once.

Instruction I. In the first series a somewhat complicated series of categories were employed, as follows.

Heavier, which means that the second weight of any pair is perceived as heavier than the first, and that this difference is clearly perceived so that it may be given with a high degree of subjective assurance.

Guess heavier, which means that the second weight seems to be heavier than the first but that you are not sure that this difference is correct.

Equal, which means that the second weight seems equal to the first and that this similarity is clearly perceived so that it may be given with a high degree of subjective assurance.

---

[6] Fernberger, On the elimination of the two extreme intensities of the comparison stimuli in the method of constant stimuli, *Psychol. Rev.*, 21, 1914, 337-340; Interdependence of judgments within the series for the method of constant stimuli, *J. Exper. Psychol.*, 3, 1920, 126-150.

Guess Equal, which means that the two weights seem to be equal or that you are unable to indicate that there is a difference, but that you are not sure that the similarity is correct.

Guess lighter, which means that the second weight seems to be lighter than the first but that you are not sure that the difference is correct.

Lighter, which means that the second weight is lighter than the first and that this difference is so clearly perceived that you may give the judgment with a high degree of subjective assurance.

Do not know, which means that you are unable to make any judgment or that your judgment is made with such a low degree of subjective assurance that you are not even willing to hazard a guess.

Instruction II. The categories for the second series were as follows, with only two categories prescribed.

Lighter, which means that the second weight is lighter than the first.

Heavier, which means that the second weight is heavier than the first.

If there should be a case in which you are unable to tell whether or not the second weight is either lighter or heavier than the first, you will indicate this by saying *Do not know*, but in every case a real effort should be made to give a difference judgment.

Instruction III. The instructions for the third series involved three categories of judgment defined as follows.

Heavier, which means that the second weight of any pair is heavier than the first.

Equal, which means that the second weight of any pair is equal to the first.

Lighter, which means that the second weight of any pair is lighter than the first.

Instructions IV. The final series defined the same three categories as follows.

Lighter, which means that the second weight of any pair seems lighter than the first and that you are perfectly sure of this judgment.

Heavier, which means that the second weight of any pair seems heavier than the first and that you are perfectly sure of this judgment.

Equal, which means that the second weight of any pair seems equal to the first, whether you are sure of the similarity or not, and also all cases of possible difference judgments of which you are so unsure that you cannot report them as heavier or lighter under the instructions given above.

Remember that you are not to give a judgment of lighter or heavier unless you are perfectly sure of this judgment.

These instructions were read to every *O* several times before each experimental sitting. All of the *Os* report that they had no difficulty in making judgments in accordance with each of the four instructions. For *K* and *B*, the untrained *Os*, 5 groups of 50 judgments in each pair of comparison weights were taken under Instructions I. For these *Os* in the other series and for *A* and *Ob* in all four series, 4 groups of the same size were obtained. The present study is, therefore, based on 23,100 individual judgments.

Treatment of Results

All of the thresholds and other significant values were calculated in accordance with the Φ (γ)-hypothesis by the methods developed by Urban and with the use of Urban's tables.

Under Instruction I, which involved the use of seven categories of judgments, only 7 judgments of *Do not know* were given out of a total of 6,300 chances. This number is too insignificant to be given individual statistical treatment and these

judgments were recorded in the final tables of the observed relative frequencies and treated statistically as *Guess equal*.

Of the six remaining categories the three following groupings seem obvious and meaningful. The significant values were calculated for all of these arrangements of the data.

Treatment	*Lower Group*	*Middle Group*	*Upper Group*
IA	L, GL, ½ GE	E	½ GE, GH, H
IB	L, GL	E, GE	GH, H
IC	L	GL, E, GE, GH	H

Instruction II, which prescribed only the two extreme categories and which quite incidentally slipped in a *Do not know* as the middle category, was developed in an attempt experimentally to produce the attitude which would be indicated by the grouping IA. In the case of Instruction II the results were treated statistically for *Do not know* as if they had been equal.

Instruction III, which prescribed three categories of judgment without reference to degrees of subjective assurance, was framed in an attempt to create the attitude defined by the analysis IB. Instruction III is the usual instruction most frequently given in psychophysical work.

Instruction IV, in which the necessary high degree of subjective assurance for the difference judgments was emphasized, was framed in an attempt to duplicate the situation analysed by IC. Thus it seemed reasonable to believe that we could compare formal or calculated and experimental results for each of these three possible attitudes.

Results

The results are given in Tables I-XII. All of the tables have a similar form. In the upper half of each table will be found the formal values calculated from the appropriate arrangement of judgments taken under Instruction I and in the lower half will be found the experimental values taken under the appropriate instructions framed to duplicate the situation under Instruction I defined by the grouping used in the same table. In the first columns are indicated the series and instructions. The next two columns contain the values of h_1 and h_2 which may respectively be considered as indices of precision for the lower and upper psychometric functions. The values of S_1 and S_2, the lower and upper thresholds, will be found in the next two columns. The next column contains the values of the interval of uncertainty, defined as $S_2 - S_1$. In the last column are the values for the point of subjective equality. The averages are given for each half of every table.

Three tables are given to the results of every *O*. In the first will be found the results for Instruction IA and Instruction II (two extreme categories and *Do not know*). In the second table will be found the results for Instruction IB and Instruction III (three

TABLE I

OBSERVER *K*; INSTRUCTIONS IA AND II

	Series	h_1	h_2	S_1	S_2	Interval of uncertainty	Point of subjective equality
FORMAL	I1A	0.046	0.062	92.31	102.62	10.31	98.20
	I2A	0.054	0.060	93.17	99.93	6.76	96.73
	I3A	0.069	0.076	94.09	97.13	3.06	95.69
	I4A	0.081	0.084	96.17	96.74	0.57	96.46
	I5A	0.143	0.138	96.95	97.55	0.60	97.34
	Ave.	0.079	0.084	94.54	98.80	4.26	96.86
EXPERIMENTAL	II1	0.161	0.161	97.09	97.09	0.00	97.09
	II2	0.125	0.126	93.34	95.34	2.00	94.35
	II3	0.149	0.120	94.41	97.52	3.11	95.80
	II4	0.133	0.123	94.03	96.06	2.03	95.01
	Ave.	0.142	0.132	94.72	96.50	1.78	95.56

TABLE II

OBSERVER *K*; INSTRUCTIONS IB AND III

	Series	h_1	h_2	S_1	S_2	Interval of uncertainty	Point of subjective equality
FORMAL	I1B	0.051	0.056	91.11	104.11	13.00	97.94
	I2B	0.058	0.067	91.24	101.38	10.14	96.66
	I3B	0.070	0.075	92.95	98.58	5.63	95.88
	I4B	0.084	0.083	95.36	97.56	2.20	96.46
	I5B	0.139	0.181	96.11	98.34	2.23	97.37
	Ave.	0.080	0.092	93.35	100.00	6.64	96.86
EXPERIMENTAL	III1	0.101	0.168	93.14	98.12	4.98	96.24
	III2	0.160	0.159	92.69	97.64	4.95	95.16
	III3	0.122	0.152	94.14	97.61	3.47	96.06
	III4	0.159	0.306	93.94	96.20	2.26	95.43
	Ave.	0.136	0.196	93.48	97.39	3.92	95.72

TABLE III

OBSERVER *K;* INSTRUCTIONS IC AND IV

	Series	h_1	h_2	S_1	S_2	Interval of uncertainty	Point of subjective equality
FORMAL	I1C	0.049	0.061	89.84	107.14	17.30	99.40
	I2C	0.057	0.066	91.07	102.11	11.04	96.97
	I3C	0.079	0.071	92.39	100.02	7.63	96.02
	I4C	0.076	0.074	94.62	99.57	4.95	97.06
	I5C	0.145	0.144	95.32	99.92	4.60	97.58
	Ave.	0.081	0.083	92.65	101.75	9.10	97.41
EXPERIMENTAL	IV1	0.090	0.092	88.72	101.64	12.92	95.24
	IV2	0.072	0.129	90.95	100.16	9.21	96.85
	IV3	0.108	0.149	89.59	99.88	10.29	95.56
	IV4	0.068	0.106	90.29	99.06	8.77	95.62
	Ave.	0.084	0.119	89.89	100.18	10.30	95.82

TABLE IV

OBSERVER *B;* INSTRUCTIONS IA AND II

	Series	h_1	h_2	S_1	S_2	Interval of uncertainty	Point of subjective equality
FORMAL	I1A	0.055	0.058	98.17	100.42	2.25	99.32
	I2A	0.079	0.076	98.90	99.53	0.63	99.21
	I3A	0.084	0.084	98.43	99.11	0.68	98.77
	I4A	0.089	0.089	97.50	98.47	0.97	97.98
	I5A	0.084	0.072	96.29	98.86	2.57	97.48
	Ave.	0.078	0.076	97.86	99.28	1.42	98.55
EXPERIMENTAL	II1	0.090	0.085	94.73	97.53	2.80	96.10
	II2	0.082	0.087	94.80	97.60	2.80	96.24
	II3	0.091	0.088	95.04	97.70	2.66	96.35
	II4	0.097	0.086	94.04	96.55	2.51	95.22
	Ave.	0.090	0.086	94.65	97.34	2.69	95.98

TABLE V

OBSERVER *B;* INSTRUCTIONS IB AND III

	Series	h_1	h_2	S_1	S_2	Interval of uncertainty	Point of subjective equality
FORMAL	I1B	0.056	0.064	94.86	103.37	8.51	99.41
	I2B	0.078	0.080	97.63	100.85	3.22	99.22
	I3B	0.082	0.085	97.07	100.58	3.51	98.85
	I4B	0.088	0.091	96.71	99.30	2.59	98.03
	I5B	0.088	0.071	95.25	99.96	4.71	97.34
	Ave.	0.078	0.078	96.30	100.81	4.51	98.57
EXPERIMENTAL	III1	0.127	0.115	94.35	96.54	2.19	95.39
	III2	0.120	0.097	95.18	97.80	2.62	96.36
	III4	0.094	0.097	93.83	97.99	4.16	95.95
	III5	0.109	0.121	96.04	99.07	3.03	97.63
	Ave.	0.112	0.108	94.85	97.85	3.00	96.33

TABLE VI

OBSERVER *B;* INSTRUCTIONS IC AND IV

	Series	h_1	h_2	S_1	S_2	Interval of uncertainty	Point of subjective equality
FORMAL	I1C	0.055	0.073	93.01	105.14	12.13	99.91
	I2C	0.075	0.074	94.10	103.64	9.54	98.83
	I3C	0.081	0.088	94.87	102.80	7.93	99.00
	I4C	0.079	0.088	93.62	101.80	8.18	97.94
	I5C	0.086	0.075	92.52	103.25	10.73	97.54
	Ave.	0.075	0.080	93.62	103.33	9.70	98.64
EXPERIMENTAL	IV1	0.097	0.075	91.88	101.02	9.14	95.86
	IV2	0.101	0.098	91.98	100.63	8.65	96.24
	IV3	0.090	0.077	88.94	100.61	11.67	94.32
	IV4	0.123	0.081	91.02	101.90	10.88	95.35
	Ave.	0.103	0.083	90.96	101.04	10.08	95.44

TABLE VII

OBSERVER *A*; INSTRUCTIONS IA AND II

	Series	h_1	h_2	S_1	S_2	Interval of uncertainty	Point of subjective equality
FORMAL	I1A	0.078	0.079	95.22	97.74	2.52	96.49
	I2A	0.080	0.081	94.04	98.42	4.38	96.24
	I3A	0.087	0.093	93.83	94.90	1.07	94.38
	I4A	0.101	0.090	95.01	96.62	1.61	95.77
	Ave.	0.086	0.086	94.52	96.92	2.40	95.72
EXPERIMENTAL	II1	0.110	0.110	96.29	96.29	0.00	96.29
	II2	0.106	0.108	92.87	95.90	3.03	94.41
	II3	0.091	0.095	94.59	97.88	3.29	96.27
	II4	0.101	0.102	94.48	97.07	2.59	95.78
	Ave.	0.102	0.104	94.56	96.78	2.23	95.69

TABLE VIII

OBSERVER *A*; INSTRUCTIONS IB AND III

	Series	h_1	h_2	S_1	S_2	Interval of uncertainty	Point of subjective equality
FORMAL	I1B	0.081	0.079	94.15	98.82	4.67	96.46
	I2B	0.083	0.082	95.47	99.14	3.67	96.22
	I3B	0.093	0.095	92.42	96.10	3.68	94.29
	I4B	0.094	0.090	94.23	97.87	3.64	96.00
	Ave.	0.088	0.086	94.07	97.98	3.92	95.74
EXPERIMENTAL	III1	0.100	0.095	94.39	99.87	5.48	97.05
	III2	0.137	0.128	94.77	99.76	4.99	97.17
	III3	0.127	0.126	94.11	98.84	4.73	96.47
	III4	0.111	0.121	93.45	97.74	4.29	95.68
	Ave.	0.119	0.118	94.18	99.05	4.87	96.59

TABLE IX

OBSERVER *A*; INSTRUCTIONS IC AND IV

	Series	h_1	h_2	S_1	S_2	Interval of uncertainty	Point of subjective equality
FORMAL	I1C	0.077	0.085	93.32	100.33	7.01	96.98
	I2C	0.087	0.089	92.66	101.56	8.90	97.17
	I3C	0.103	0.093	90.90	98.46	7.56	94.49
	I4C	0.093	0.088	93.04	100.46	7.42	96.65
	Ave.	0.090	0.089	92.48	100.20	7.72	96.32
EXPERIMENTAL	IV1	0.068	0.090	88.41	103.41	15.22	97.10
	IV2	0.098	0.108	90.80	101.76	10.96	96.52
	IV3	0.096	0.074	92.48	107.13	14.65	98.85
	IV4	0.097	0.100	92.07	101.95	9.88	97.09
	Ave.	0.090	0.093	90.94	103.62	12.68	97.39

TABLE X

OBSERVER *Ob*; INSTRUCTIONS IA AND II

	Series	h_1	h_2	S_1	S_2	Interval of uncertainty	Point of subjective equality
FORMAL	I1A	0.109	0.107	91.40	96.32	4.92	93.83
	I2A	0.083	0.073	90.81	95.17	4.36	92.86
	I3A	0.081	0.078	89.01	96.25	7.24	92.56
	I4A	0.112	0.094	90.38	94.91	4.53	92.45
	Ave.	0.096	0.088	90.40	95.66	5.26	92.92
EXPERIMENTAL	II1	0.112	0.105	90.87	91.37	1.03	91.37
	II2	0.116	0.104	91.12	92.21	1.09	91.60
	II3	0.097	0.096	90.24	93.53	3.29	91.88
	II4	0.132	0.115	91.15	92.90	1.75	91.97
	Ave.	0.114	0.105	90.84	92.50	1.79	91.70

TABLE XI

OBSERVER *Ob;* INSTRUCTIONS IB AND III

	Series	h_1	h_2	S_1	S_2	Interval of uncertainty	Point of subjective equality
FORMAL	I1B	0.112	0.108	90.99	96.60	5.61	93.61
	I2B	0.090	0.072	90.05	96.09	6.04	92.73
	I3B	0.080	0.084	88.18	96.91	8.73	92.66
	I4B	0.111	0.099	89.82	95.40	5.58	92.45
	Ave.	0.098	0.091	89.76	96.25	6.49	92.86
EXPERIMENTAL	III1	0.132	0.117	90.76	95.76	5.00	93.10
	III2	0.094	0.090	90.69	97.57	6.88	94.04
	III3	0.094	0.108	90.83	96.23	5.40	93.71
	III4	0.091	0.089	89.44	94.40	4.96	91.90
	Ave.	0.103	0.101	90.43	95.99	5.56	93.19

TABLE XII

OBSERVER *Ob;* INSTRUCTIONS IC AND IV

	Series	h_1	h_2	S_1	S_2	Interval of uncertainty	Point of subjective equality
FORMAL	I1C	0.111	0.103	90.67	97.98	7.21	94.20
	I2C	0.090	0.072	89.53	98.16	8.63	92.99
	I3C	0.085	0.082	87.98	97.36	9.38	92.53
	I4C	0.114	0.098	89.58	95.48	5.90	92.30
	Ave.	0.100	0.089	89.44	97.24	7.78	93.00
EXPERIMENTAL	IV1	0.107	0.101	90.10	95.44	5.34	92.70
	IV2	0.099	0.110	89.25	96.42	7.17	93.02
	IV3	0.131	0.118	89.74	96.27	6.53	92.82
	IV4	0.115	0.097	89.74	95.34	5.60	92.31
	Ave.	0.113	0.106	89.71	95.87	6.16	92.72

categories relatively undefined). In the third table will be found the results for Instruction IC and Instruction IV (three categories with emphasis on strong subjective assurance for the extreme categories).

Progressive Practice

Before we proceed to our primary problem of the effect of instructions, it seems worth while for us to consider the effects of progressive practice. It will be remembered that Instruction I (six categories plus *Do not know*) were given first to all *O*s. It will also be remembered that *A* and *Ob* were highly practiced at the start of the experiment, and that *K* and *B* were entirely untrained and that their judgments were recorded from the beginning of practice. For the present study it is necessary to consider only the upper halves of the tables.

A comparison of the values in the upper halves of the tables for *Ob* (Tables X-XII) indicates great constancy of values within each table. This fact indicates that, for this highly trained *O*, all of the six categories of judgment were well defined at the beginning of the experiment and that these definitions remained constant throughout the experiment.

To a slightly lesser degree, the same relation holds for *A* (Tables VII-IX). His definition of the two extreme categories seems to be well established inasmuch as the values remain constant under arrangement IC (Table IX). For arrangement IB (Table VIII) the values again remain relatively constant. There is a decrease in the size of the interval of uncertainty for arrangement IA (Table VII) which indicates a decrease in the number of equal judgments. Inasmuch as the values for IB remain constant, the decrease in the number of equal judgments must have been accompanied by a relatively similar increase in the number of *Guess equal* judgments. We can conclude, however, that for the trained *O*s the six categories of judgment were relatively well defined at the start of the experiment and remain relatively constant throughout the experiment.

A very different situation is found for those *O*s who were untrained at the beginning of the experimentation and in whom we may expect progressive practice. In the case of *K* (Table I) for grouping IA, there is a marked decrease in the size of the interval of uncertainty which indicates, under this arrangement of data, a marked decrease in the number of equality judgments given with a high degree of subjective assurance. The value of the point of subjective equality remains relatively unchanged. For grouping IB of *K*, in which the

Guess equals are calculated with the *Equals*, one finds again a marked decrease in the size of the interval of uncertainty with a relatively constant value for the point of subjective equality. The courses of the values for the intervals of uncertainty for arrangements IA and IB remain relatively parallel, a relation which indicates that, although the relative number of *Equal* judgments decreased markedly, the number of *Guess equal* judgments remained practically unchanged throughout the initial progressive practice. For *K* under arrangement IC, in which all *Guess* judgments were calculated with the *Equal* judgments, one again finds a marked decrease in the size of the interval of uncertainty with a relative constancy of the size of the point of subjective equality. The initial drop for arrangement IC is more rapid than that for either IA or IB. We found above that the number of *Guess equal* judgments remained relatively constant while the number of *Equal* judgments markedly decreased. The more rapid initial drop in arrangement IC indicates that there must have been a decrease in the number of *Guess lighter* and *Guess heavier* judgments as well.

The results for *B* (Tables IV-VI) present a quite different picture, largely due to the presence of an inversion in the last series. In every case there is an initial drop. This is least for arrangement IA indicating only a slight decrease in the number of *Equal* judgments with progressive practice. It is greatest for arrangement IB and, when one considers the relatively slight decrease in *Equal* judgments found for arrangement IA, this indicates a marked decrease in the number of *Guess equal* judgments as the result of progressive practice. The relative number of *Guess lighter* and *Guess* heavier judgments (Table VI) for arrangement IC seems to remain relatively constant. For this *O* there is a slight decrease in the size of the point of subjective equality throughout.

For both *K* and *B* there is an increase in the size of the indices of precision (h_1 and h_2) for all arrangements. These results, namely, the increase in the size of the indices of precision, the marked decrease in the size of the intervals of uncertainty and, to a lesser degree, of the points of subjective equality, completely agree with earlier work on the effects of progressive practice.[7] The present study, however,

[7] F. M. Urban, Der Einfluss der Übung bei Gewichtsversuchen, *Arch. f. d. ges. Psychol.*, 29, 1913, 271-311; Fernberger, On the relation of the methods of just perceptible differences and constant stimuli, *Psychol. Monog.*, 14, 1913, (no. 4), 19-46; The effects of practice in its initial stages in lifted weight experiments and its bearing upon anthropometric measurements, this JOURNAL, 27, 1916, 261-272.

gives a more detailed indication of how these changes are effected. The study of the different values indicates the shift of the *Equality* and *Guess lighter* and *Guess heavier* judgments to the judgments of difference. The increase in the size of the indices of precision, indicating a steeper curve for the psychometric functions of the *Lighter* and *Heavier* judgments indicates that this shift is not evenly distributed throughout these curves. It indicates that relatively more of these judgments have shifted to the upper parts of the curves in both cases, thus making both curves steeper. Finally, inasmuch as the values of h have been increased for both curves of the psychometric functions for the lighter and heavier judgments in about equal amount, it means that the shift has been in about the same proportion to both extremes of judgment.

The Effect of Instructions

It is obvious that our formal distributions IA, IB, and IC (the upper half of all tables) will give different values for the interval of uncertainty for any *O*. This is true by definition inasmuch as we included different categories of judgment for each arrangement in the middle statistical category which defines the size of the interval of uncertainty. For arrangement IA we included only the positively *Equal* judgments. In IB we added to these the *Guess equal* judgments and in IC we still further added the *Guess lighter* and *Guess heavier* judgments. Hence, by definition, the order of the magnitude of the interval of uncertainty under the formal arrangements must be IA, IB, and IC.

There are marked differences in the relative average size of these values, however, for the different *O*s. For *K*, the intervals for IA and IB are relatively not so different as that for IC is from either of the others. This means that there were considerably more *Guess heavier* and *Guess lighter* judgments than *Guess equal* judgments for this *O*. The same situation is true, to a somewhat lesser degree, for *B*, with a greater relative difference in the size of the interval of uncertainty of IC as compared with IB than between IB and IA. It will be remembered that *K* and *B* were untrained in the lifted-weight procedure at the beginning of the experiment.

For *Ob* the same relation holds but to a much less marked degree than for even *A;* and for *Ob* there is a constant relation of increase for the three arrangements. This increase would indicate that, for these trained *O*s, there are relatively fewer *Guess lighter* and *Guess heavier* judgments than for the untrained *O*s. It would seem, then,

as we compare the trained and untrained *O*s, as if the 'guess' type of judgment was eliminated for all categories of judgment as the result of progressive practice and as if the *Guess lighter* and *Guess heavier* judgments tended to be eliminated before the *Guess equal.*

Instructions II, III, and IV were attempts to create attitudes in the *O*s which would correspond respectively to the formal arrangements IA, IB, and IC. For a consideration of these data, compare the top and bottom halves of each table.

These results indicate that we have been relatively successful in the attempt to create attitudes which would give experimental results comparable to the three different formal arrangements. The rank order of the size of the interval of uncertainty for the formal series is, by definition, IA, IB, and IC. For every *O*, the rank order under the different experimental instructions is II, III, and IV.

The relation of the average size of the interval of uncertainty, however, varies for the different *O*s. For *K*, the interval for Instruction III is slightly larger than for II, while that for Instruction IV is much larger than for either. For *B*, the values for II and III are almost identical, while that for IV is much larger than for either of the others. The same is true for *A*. A somewhat different picture is found for *Ob*, for whom III and IV closely resemble one another in magnitude while both are considerable larger than the interval of uncertainty for Instruction II.

When one compares the formal and experimental values of the interval of uncertainty for any individual *O*, one finds that they are of reasonably the same magnitude. For *K* (Tables I-III) the experimental values are constantly smaller than the formal ones. This is largely due to the fact that we have included the results of early practice in the averages under the different arrangements of instruction I. If the first two series are eliminated and the last three formal series are averaged alone, the formal and experimental results for IA and II and for IB and III are almost identical. For this *O*, in the first series under Instruction II, we find no *Do not know* judgments given at all. Hence the upper and lower thresholds coincide and the interval of uncertainty becomes zero. The results indicate, however, that it was not possible to maintain this attitude in its extreme form.[8] A similar result is found for *A*.

[8]Professor Warner Brown has suggested to the writer that the difficulty in maintaining this extreme attitude of the elimination of all equality or *Do not know* judgments was due to the primary training in giving *Equal* judgments for this *O*.

For *B* (Tables IV-VI) the formal and experimental values of the interval of uncertainty are in all three cases almost identical. The experimental values are higher for the comparison of IA and II and for IC and IV, and lower for IB and III.

For *A* (Tables VII-IX) the values of the interval of uncertainty are invariably higher for the experimental values than for the formal. For Instructions IA and II and for IB and III, these values are almost identical. The experimental value for Instructions IV is considerably greater than the corresponding formal arrangement IC.

For *Ob* (Tables X-XII) the formal values are invariably higher, for the interval of uncertainty, than the experimental. For arrangement IA and II the difference is marked. For the other two comparisons (IB-III and IC-IV) the approximation is very close.

For all of the *O*s the change in formal grouping under arrangements IA, IB, and IC has little or no appreciable effect upon the size of the point of subjective equality. The same may be said with regard to the point of subjective equality under the different instructions II, III, and IV. There is practically no change of the magnitude of this value for any of the three instructional attitudes.

For *A* and *Ob*, the trained *O*s, the values of the points of subjective equality are almost identical for each of the three experimental and formal comparisons as well. For *B* and *K*, the untrained *O*s, the values for the formal points of subjective equality are invariably higher than their corresponding experimental values. This latter result was to be expected inasmuch as the formal values were taken first (Instructions I) and we know that progressive practice effects a lowering of the point of subjective equality which, for these *O*s, seems to have been established by the time of all of the later experimental instructions.

It seems to the author that these results should have a considerable effect upon the theory of anthropometric measurements and of test procedures in particular where questions of sensitivity are involved. He does not, however, wish to discuss this feature in the present paper.

Conclusions

(1) The results indicate that the present method of formal analysis for the calculation of judgments in a number of categories promises to give an adequate analysis of the effects of progressive practice on the different categories of judgment. The analyses of the

effects of practice on many more than the two untrained *O*s here reported are needed.

(2) The results also indicate that it is possible to frame instructions which will define the middle category for the method of constant stimulus difference so that there will be included in this middle category, in one case only those equality judgments given with a high degree of subjective assurance; in a second case, all equality judgments whether given with a high degree of subjective assurance or not; and, in a third case, all equality judgments and all of the difference judgments given with a relatively low degree of subjective assurance.

(3) These three instructions seem to be the basis of an attitude readily assumed by the *O*s and adequately maintained throughout the experimental periods.

(4) The first attitude gives the smallest interval of uncertainty and the last gives the largest.

(5) The three instructionally induced attitudes do not seem to have any effect upon the magnitude of the point of subjective equality.

(6) These results clearly indicate the necessity for care in the formulation of instructions, for the printing of the actual instructions, and for a determination of the attitude (interpretation of the instructions) of the *O* in all psychophysical work.

(7) Some of the individual differences between different *O*s in older psychophysical work may possibly be accounted for in terms of this difference in attitude. The usual instructions given in the past seem to produce what we have here obtained from Instruction III (all *Equal* judgments in the central category). There seems to be the possibility that some *O*s may have *interpreted* these intermediate instructions in either direction—thus producing attitudes corresponding to our Instruction II or IV.

(8) If one may hazard a guess, these differences in the interpretation of instructions may be due to temperamental differences in the *O*s, as was suggested by Frank Angell more than twenty years ago.

CORRELATION OF REACTION AND COÖRDINATION SPEED WITH AGE IN ADULTS

By W. R. MILES, Stanford University

Speed of reaction, the rapidity with which an individual initiates movements after perceiving certain stimuli, and speed of coördination, the promptness with which total acts are carried through, may be tremendously important either in the more or less of a person's hourly output, or in the all or none situations presented, *e.g.*, in industry by the operation of power machines, or in everyday activities such as walking through city traffic.[1] The celerity with which a given person accomplishes any given act depends of course upon many conditions both objective and subjective. But even without attempting to control these, if we observe an individual for a time we may form a general impression of his tempo. We may then say that he is average, like most other people; or deliberate, slow, weak from illness, sleepy, perhaps mentally confused; or again that he is wide awake, quick on the trigger, physically and mentally alert. Physicians give attention to locomotion, speech, gestures, simple manipulations, etc., as symptomatic behavior significant for the understanding of their patients. The neurologist depends upon the promptness and responsiveness of reflexes to indicate condition of health, or interference in the nervous system. Similarly psychologists both experimental and applied place confidence in specified motor coördinations and reactions.[2] If a subject is coöperative, understands the required task and how he is to do it, then timed results, compared with those secured under similar objective circumstances from other persons, may be indicative of important behavior tendencies. Such results serve as symptoms, as placement indices, as predictive bases, and as indicators of intelligence.

A good although quite specialized example of the symptomatic value of reaction-time is given in the work of Macht and Isaacs on the effect of opium alkaloids.[3] Determination of degrees of reactive slowness and muscular incoördination have served importantly in a study on the influence of a marked and prolonged reduction in diet[4] and, as is well-known, 'voluntary motion' is regularly considered symptomatic in medical diagnosis.[5] Manual tests of the dexterity

*Accepted for publication March 31, 1931.

[1]Part of an extended study on Later Maturity conducted in the Psychology Department of Stanford University and made possible through a grant from the Carnegie Corporation of New York.

[2]H. M. Johnson, Reaction-time measurements, *Psychol. Bull.*, 20, 1923, 562-589.

[3]D. I. Macht and S. Isaacs, The action of some opium alkaloids on the psychological reaction-time, *Psychobiol.*, 1, 1917, 19-32.

[4]F. G. Benedict, W. R. Miles, P. Roth, and H. M. Smith, Human vitality and efficiency under prolonged restricted diet, *Carnegie Inst. of Wash. Pub.*, 280, 1919. See Psychological Sections, 139-192, 551-638.

[5]R. Bing, *Compendium of Regional Diagnosis in Affections of the Brain and Spinal Cord*, 1921.

of industrial workers ordinarily give strong emphasis to reaction and coördination speed. Such tests have proven useful in the placement of industrial workers. A recent example from this wide field is the study by Walther who found correlations of +0.80 or above between the test scores and output in workers who had held their positions for several years.[6] Speed and coördination were largely involved in dexterity tests as applied to specific trades for placement and predictive purposes made in the United States Army shortly after 1917. Results on 250,000 men who claimed skill in one occupation or another showed "that 6% of them really possessed the expert skill claimed, 24% of them possessed journeyman skill, 40% of them had the equivalent of apprenticeship knowledge of their trade, 30% knew nothing whatever of the trades in which they claimed expertness."[7]

In restricted fields the factor of speed is preëminent, for example, in athletics. In measuring the charging time of football players by the use of a multiple chronograph which recorded for all seven men of the line at once,[8] the writer found that six of the first team men had times below 0.4 sec., while six of the second team line had times greater than that value. When asked, "Are the men on the first team fast because they are motivated to try and hold their places on that team, or are they on the first team because they are fast?" the coach promptly replied, "They are on the first team because they are naturally fast."

Since the beginning of mental testing both individual and group examinations have directly or indirectly made much use of the factor of speed. Peak and Boring,[9] and more recently McFarland,[10] have presented results which show positive correlation between general reaction speed and degree of accomplishment in achievement and intelligence tests. These experimental findings seem well in accord with observations on vocational and professional activity. The individuals who can work out successfully the largest number of problems or items in a well constructed power test of mental ability, the item range of which extends beyond the capacity of those being examined, are probably those whose general working speed under pressure is also greatest.

This brief inventory of some of the places where speed of reaction and coördination are significant indicates the wide extent to which it is generally considered important not only to function well but also to act with greater rather than less promptness. Speed of reaction is in itself an important trait of behavior. It is in general an advantage to a dull mind to operate more rapidly than other dull minds. It is ordinarily a disadvantage to a bright mind when activity is slowed down, whatever the cause. Children may be ever so bright, yet slow in certain reactions because of inexperience. Similarly, novices of any age are in general slower in successful accomplishment than are the more experienced workers.

---

[6] L. Walther, Technopsychology in a Swiss industry, *Personnel Journal*, 8, 1930, 1-18.

[7] W. D. Scott and R. C. Clothier, *Personnel Management Principles, Practices, and Point of View*, 1923, 96 ff. See also Scott's *Manual Dexterity Test*, 286.

[8] W. R. Miles, Studies in physical exertion: I. A multiple chronograph for measuring groups of men, *Amer. Physical Educ. Rev.*, 33, 1928, 379-387.

[9] H. Peak and E. G. Boring, The factor of speed in intelligence, *J. Exper. Psychol.*, 9, 1926, 71-94.

[10] Ross A. McFarland, The rôle of speed in mental ability, *Psychol. Bull.*, 25, 1928, 595-612; An experimental study of the relationship between speed and mental ability, *J. Gen. Psychol.*, 3, 1930, 67-97.

A group of workers of considerable economic importance who are said to be regularly slower than other persons similar to them are men and women past middle life. The common impression that age brings a slowing of performance is based largely on industrial experience and on general observations of adults at work and at play. In contrast to this belief is reported the fact that Wundt, using a demonstration of reaction-time in connection with his lectures in which he himself served as subject, was able from year to year and until late in life to respond as promptly as he had done at the beginning of his career. There is probably no question at all that such individual records as these of the great experimenter are by no means typical of aging men and women in general. But it it equally clear that such slowness as Exner reported for a man of 72 years (1.00 sec. for simple reaction) is not necessarily typical for that age.[11] The subject in Exner's classical experiment was as a matter of fact a pauper and below average in intelligence and neuro-muscular capacity to act.[12]

Although reaction-time and coördination speed have often been carefully measured and reported upon for the periods of youth and early adulthood, data for the period of later maturity have not been accumulated to any considerable extent. Two things have operated to hinder experimental work with older *S*s. In the first place older people, unless themselves trained along experimental lines, have not been interested in measurements of this kind, and furthermore they have not been readily available as *S*s in psychological laboratories. The bulk of the early reaction-time studies were carried on almost exclusively in university laboratories where instructors and students worked largely on themselves and their associates, with the result that the methods were constantly further refined while the subject material remained about the same. Later, as the growth and development of childhood and youth became special objectives for the accumulation of psychological data and especially that which helped to define the possibilities of learning, school children of various ages were put through the different series of experiments. It was then possible to compute reaction norms for the ages from early childhood to adulthood as far as the school and college populations were concerned. Data for representative general populations and more advanced age groups were still wanting.

[11]S. Exner, Experimentelle Untersuchung der einfachsten psychischen Processe, *Pflüger's Archiv*, 7, 1873, 601-660; see particularly pages 612-14.

[12]S. Exner, Physiologie der Grosshirnrinde, Hermann's *Handbuch d. Physiol.*, Vol. 2, 1897, 189-350. Exner gave here a fuller account of his work with the old pauper. The dates of measurement and mean reaction-times found were: June 21, 1872, 1.00 sec.; July 1, 1872, 0.36 sec.; January 9, 1873, 0.19 sec. Thus it appears that the dull old man did exhibit marked improvement with practice. The fastest reaction-time found by Exner was 0.129 sec. for a young man of 22 years.

The second drawback to a collection of data on older adults has arisen from the variety of apparatus, routine, and practice in use in various laboratories. Simple reaction-times measured by different equipment and by various experimenters are theoretically comparable but practically experimentation with a given technique and procedure and under given conditions with a specific routine and a large group of subjects representing the full age range is far more satisfying than an attempt to piece together results from many workers. A dependable timing equipment, as uncomplicated as possible, and a routine that supplies subjects regularly and promptly one after the other and carries them through the same amount of practice and with as nearly equal motivation as possible are highly desirable conditions for such a study.

In this paper I am reporting on a sample group of one hundred adults, taken without selection from the individuals in a small suburban population, who came in as subjects in a general study of later maturity. These subjects represented both sexes and probably covered the range of literate members of the community.[13] The apparatus, the procedure followed, and some of the results are discussed in the following pages.

Apparatus and Procedure

Alternating current timing apparatus. Simplicity, portability, and low cost are usual considerations in determining the choice of laboratory equipment.[14] I would not claim that the unit here described is the ultimate one for coördination and reaction-time work, or that it is the best all-round set now available. It seems, however, to qualify as quite generally usable, both within the laboratory and outside it, at factory, private house, school or playground, *i.e.* wherever 60-cycle, 110 v. current is available. Probably for most purposes it offers a small enough time-unit and a safely accurate and rugged mechanism.

The central feature of this reaction-board unit, first used in the Stanford Laboratory in 1929, is an electrical impulse counter designed by Klopsteg.[15] I understand that Klopsteg had no thought of reaction-time when he designed this counter, and so far as I know it has not been used as a reaction-timer except

[13]The subjects were sent to the Psychology Annex, a cottage conveniently situated in the town, by church organizations, lodges, and other clubs, who received a bounty for each subject who went through the routine (two cents a year if the subject was fifty or older, and twenty-five cents extra for each man). The subjects themselves received no payment for their coöperation. They served for the benefit of science and in aid of the organization that secured them.

[14]The apparatus as described and illustrated in this paper was demonstrated at a meeting of the Society of Experimental Psychologists, Cornell University, Ithaca, N. Y., April 18, 1930.

[15]P. E. Klopsteg, An impulse counter, tallying device, and electric 'stop-watch': a useful laboratory instrument, *J. Optical Soc. of Amer. & Rev. of Sci. Inst.*, 19, 1929, 345-349.

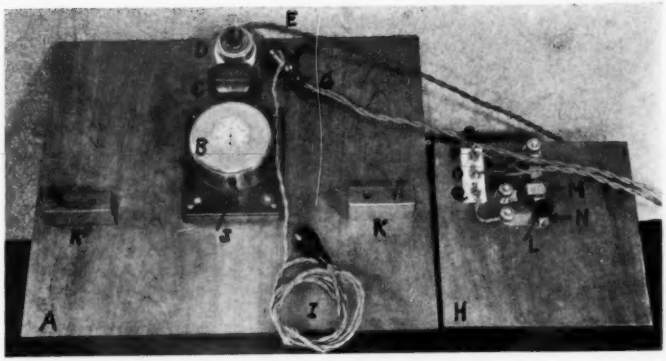


FIG. 1. REACTION-BOARD AND ACCESSORY KEY

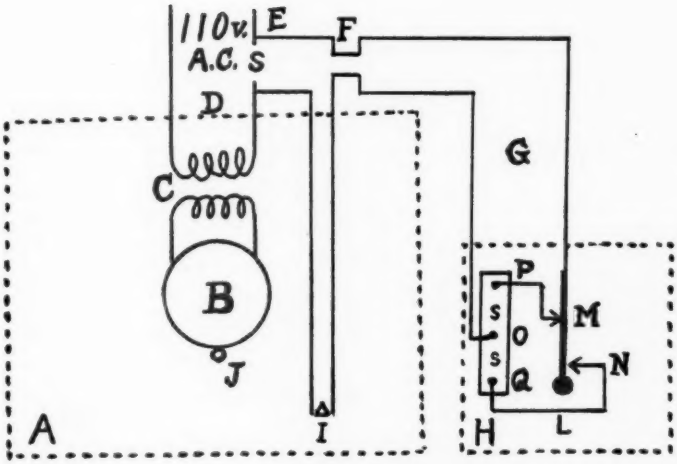


FIG. 2. SIMPLE WIRING ARRANGEMENT FOR THE APPARATUS

in the present set-up. The counter was planned and is manufactured for use in physics experiments, particularly for accurately measuring the frequency of vibrating bars, or tuning forks, as well as for measuring the number of alterations per second in alternating current circuits of 60 cycles or less. The manufacturers claim that it will respond dependably up to and including 125 impulses per sec.

This compact counter is built about an electro-magnet which when energized operates a pivoted armature. The armature is normally held against a stop by spring tension. To the armature is attached a driving pawl, which at each stroke of the armature toward the magnet advances a 60-tooth ratchet wheel one step. The motion of the ratchet wheel is rendered positive by a check pawl. This device locks the driving pawl when it is in this advanced position. The mechanism, because of its low moment of inertia and its strong actuating power, operates positively without skips at all frequencies below 125 per sec. "Light moving parts, properly adjusted, and sufficiently strong impulses, make this surprising performance possible." A sweep-second hand indicates individual impulses on a 60-division dial. There is a small auxiliary hand which counts revolutions of the sweep-second hand. When the impulse counter is operated on the 60-cycle current, now commonly employed for time clocks, it receives 120 impulses per sec., and therefore the large hand makes two complete revolutions per sec., and each unit on the dial represents 1/120 sec. The impulse counter is operated from the secondary of a 12-v. bell-ringing transformer. There is a control key in the center of the base at the front of the counter which when pressed mechanically stops the movement of the armature. This key may be used in reaction-tests and in coördination-timing, as will be explained later.

The entire reaction-board set-up is illustrated in Fig. 1. The impulse counter, (B), is located near the center of a three-ply panel, (A), which is 18 in. wide from left to right and 15 in. deep, front to back. (In use the panel is placed flat on the table and in front of the person being tested.) The small transformer, (C), is secured to the panel behind and in close proximity to the counter. The transformer through a switch, (D), connects, (E), to an ordinary 110 volt A.C. lamp socket or outlet. The wiring, indicated in Fig. 2, is so arranged as to permit various combinations in the procedure. There are four ways of using the electric clock: (*1*) By closing switch (D), which short-circuits the push-button, (I), completes the transformer circuit and starts the clock, and pressure on (J) stops it. (*2*) By leaving (D) open, disconnecting (G) from (F) and inserting a short-circuiting cap at (F). Now pressure on the push-button, (I) completes the transformer circuit and starts the clock and pressure on (J) stops it. (*3*) By leaving (D) open and connecting (G) with (F) it is apparent that the push-button, (I), cannot close the circuit and start the clock unless switch (O) is connected to (P)—the contact point (M) is kept closed by the upward spring action of (L). If this is done then the clock may be started by pressure at (I) and stopped by pressure on (J). (*4*) The same as (*3*) except that the clock is stopped by pressure on (L)—(J) not used— and the switch is closed between (O) and (Q), reaction-key (L) is held down against (N), the clock is started by pressure on (I) and stopped by release of (L).

The impulse counter when in operation makes a buzzing sound and this sound, or the tactually felt vibration of the clock-cover if the *Ss* fingers are rested

against it, or the sight of the revolving hand, serves as the stimulus for the reaction. As soon as the circuit is broken, the buzzing sound stops, and the moving hand stops. It has been usual in reaction-time apparatus to employ as stimulus a single click or flash. When attention was not quite favorable this momentary character of the stimulus has sometimes caused *S* to be in doubt. In the apparatus here described this condition cannot result. The stimulus is a continuing thing, prolonged until the reaction is completed; it is primarily auditory, but it may be auditory and visual, or auditory, tactual and visual, combined.

The impulse counter is not returned to zero after each reaction, consequently in conducting experiments the consecutive readings are recorded in a vertical column (starting most conveniently at the bottom of the page) and later, by subtraction, the number of 1/120 sec. time-units for each reaction or coördination is found. The instrument has been found very trustworthy. Reactions or coördinations may be timed on it in rapid sequence and with the very minimum of manipulations. The reaction-board as a time measuring instrument is easily understood by *S*s, whether children or adults. This simplifies instruction and aids in securing good motivation.

Battery of time measurements. What might be termed a battery of reaction-tests can be carried out in rapid sequence with the apparatus described. The measures employed in the present study and which form a standard routine are as follows.

(1) *Single extension of the fore-finger.* With the right arm and wrist resting on the table the fore-finger is placed in contact with key (J), holding it down. The circuit is closed in the clock, either by switch (D) or push switch (I), and there is a gentle hum to be heard from the clock. Now *S* when ready lifts the finger from the key and brings it down again as quickly as possible. This is really one complete tap of the finger. Lifting the finger permits the already energized clock movement to start turning and pressure stops it. The distance the clock hand turns during the release of (J) is a time measure of the event. The clock is read before and after this simple movement. This is not a reaction measure but a coördination pure and simple. *S* decides himself when to lift the finger, and executes what he considers to be a quick movement. I have used ten consecutive trials as a measure of this activity.

(2) *Coördination for pencil shifting.* The *S* holds his right index finger on key (J), as in the previous test, and when ready reaches over to (K) with his right hand, grasps the pencil, lifts it from the hole where it is resting, and places it in the other nearby hole, then returns to key (J) and settles the fore-finger of this active hand on the key as quickly as possible. At the moment he starts the coördination, leaving (J), the electric clock starts, and when he arrives on (J) the clock stops, provided he does not fumble. The total time for this simple coördination of hand and arm is registered without any interference from *E*. *S* registers his own time by the act itself. Three trials are made with the right hand and three trials with the left hand alternately. The left hand uses block (K′).[16]

(3) *Reaction by pressing the finger.* This involves the use of accessory key (H). (O) and (P) are connected by the single pole double-throw switch. The *S* rests the finger (usually fore-finger of right hand) lightly on (L), so lightly that the contact point at (M) will remain pressed together due to the upward spring action of (L). Then, at a moment unknown to the *S*, and out of his view, *E* presses and holds push-button (I) which completes the circuit in the primary of the transformer, and thus starts the clock. Increased pressure on (L) breaks this circuit at (M) and stops the clock. This is a simple-reaction by flexion to sound,

[16]From the center of the key (J), to the nearest hole in (K) or (K′) is 6 in., and to the farther hole, 7.5 in.

or if the *S* is allowed to look at the dial of the clock, reaction to auditory and visual stimuli. Five such reactions are taken in rapid succession. The instrument is read after each reaction and successive readings are later substracted one from the other to find the reaction-time. *E* will of course vary the interval between ready signal and pressure on (I), and be careful that all manipulations of the push-button are out of sight of the *S*.

(4) *Reaction by lifting the finger.* This is the more common type of reaction-movement as used in laboratory tests. It is provided for in accessory key (H) by changing the single pole double-throw switch so as to connect (O) and (Q), thus placing in operation the contact point at (N). Now *S* presses rather firmly on (L), so that the contact points at (N) are closed. The circuit in the primary of the transformer is completed and the clock started when the experimenter presses push-button (I). At the start of the clock *S* releases (L) breaking the contact at (N) and registering a reaction. The experimenter, of course, as in (*4*), maintains pressure of the push-button until the reaction is completed. Five such reactions are taken in rapid succession.

(*5*). *Reaction by lifting the ball of the foot.* Key (H) is placed on the floor and the routine for (*5*) is carried out except that (L) is held down by the ball of the foot with the heel resting on the floor. A light pressure of the foot maintains contact at (N). When *E* presses (I) the clock starts and this constitutes the signal for the *S* to lift his foot quickly as he might do from the foot throttle of an automobile. Ten consecutive reactions of the right foot are taken.

Other coördinations and reaction combinations readily suggest themselves for time measurement by this simple unit.[17] The battery just outlined has been found a practical one for use with children and adults and can be carried through in about 15 min.[18]

Subjects. The results presented here are limited to data gathered on 100 adults, 53 men and 47 women. These *S*s belong to the upper social strata or more generally successful group in a small residential city. They were not otherwise selected. They were taken just in the order in which they presented themselves, without respect to occupation, education, sex, or age. In the sample group reported there are 7 men and 5 women beyond the age of 74 yr. The range in age is from 25 to 87 yr., with a mean of 48.7 yr., S.D. 15.9. The means and standard deviations for 6 coördination and reaction measures are included in Table I.

Results

Coördination and reaction-time. It is to be expected that the simple coördination of raising and lowering the right index finger

[17]For example a 'pursuit-reaction' test, which is performed as follows: The *S* holds his finger on (J), restraining the movements of the clock mechanism; he then lifts the finger, allows the clock hand to revolve, and attempts by pressure on (J) to stop the hand exactly at 60. The instructions for this test are: "Let the clock hand pass 60 once and then stop it on 60 at the next revolution." The number of divisions, plus or minus from 60, at every attempt, is read off, and 10 trials of this nature are made. This might be termed reaction or coördination. It involves the sense of rhythm and is somewhat like gun pointing, or stopping a gasoline pump at the proper index mark. For convenience I have called it "pursuit-reaction." This test was given to the *S*s, but the results are not reported here.

[18]The symmetrical arrangement of the apparatus makes it equally well adapted to right and left hands, and for right and left feet. The complete outfit can be made by following the specifications in this article, or it can be supplied through the writer.

TABLE I

RESULTS FOR COÖRDINATION AND REACTION-TESTS ON 100 UNSELECTED ADULTS MEASURED WITH THE REACTION-BOARD

(Time in seconds)

		Measure					
		(1)	(2)		(3)	(4)	(5)
	Age (Years)	Finger Coörd.	Rt. Hand Coörd.	Left Hand Coörd.	Finger Pressing	Finger Lifting	Rt. Foot Lifting
Range	26 to 87	0.083 to 0.367	0.85 to 2.10	0.90 to 2.25	0.100 to 0.817	0.158 to 0.450	0.158 to 0.408
Mean	48.7	0.159	1.21	1.36	0.241	0.227	0.242
S.D.	15.9	0.056	0.23	0.24	0.096	0.052	0.047

would prove the quickest event in the series. This act required on an average in this group 0.159 sec., S.D. 0.056 (mean of 10 trials), which corresponds very closely to a tapping rate of 6 per sec., or to the total time required for the upper eyelid to execute a completed wink. As shown by the range there were individuals who performed this coördination in a time equal to no more than half the mean and others who required as long as twice the average time. Transferring the pencil from one hole to the other (see measures 2 and 3 in Table I) requires for the right or dominant hand 1.21 sec., S.D. 0.23,[19] and for the left or subordinate hand 1.36 sec., S.D. 0.24 (mean for 3 trials). This is about eight-fold the time required for the simple finger movement. The simple reaction-time as indicated by the response of pressing the key (mean for five trials) is found to be 0.241 sec., S.D. 0.096, while releasing the key (mean of 15 trials) is accomplished in the average time of 0.227 sec., S.D. 0.052. There is doubtless some effect from practice but the difference between these two means is largely accounted for by the fact that in reaction by release of the key the *S* has a more certain preliminary position. When he is prepared to press the key for reaction he must at the beginning be careful that the finger is resting lightly (not more heavily than 100 grm.) on the key. This causes some *S*s in spite of instructions, to hold the finger above and off the key. The difficulty here is reflected in the very wide range shown for this measurement. The simple reaction-time with the foot in releasing the key (average of 10 trials) shows a mean of 0.242 sec., S.D. 0.047, which corresponds quite closely to the reaction-time found with the finger. The foot reactions coming

[19] Four of these *S*s were admittedly left-handed. Their left hand scores were thus averaged with the right hand series of the others.

last had the benefit of the slight amount of practice which our variety of time measurements provided.[20]

The reaction-time means in the table are all considerably longer than the reaction-time values ordinarily published. Nor is this entirely a matter of age. The act measured here may be called the "natural reaction." The *Ss* were quite unpracticed. Every one responded according to his own interpretation of "quick action," and his ability to execute it. The situations were simple and the motivation good. While "natural reaction-times" may not legiti-

TABLE II
CORRELATION OF REACTION AND COÖRDINATION MEASUREMENTS
RESULTS ON 100 UNSELECTED ADULTS
(Pearson r)

Measure	Age	(1)	(2)		(3)	(4)
		Finger Coörd.	Rt. Hand Coörd.	Left Hand Coörd.	Finger Pressing	Finger Lifting
(1) Finger Coörd.	0.36±.06					
(2) Rt. Hand Coörd.	0.55±.05	0.38±.06				
Left Hand Coörd.	0.40±.06	0.51±.05	0.53±.05			
(3) Finger Pressing	0.25±.06	0.43±.06	0.32±.06	0.31±.06		
(4) Finger Lifting	0.31±.06	0.29±.06	0.24±.06	0.30±.06	0.50±.05	
(5) Rt. Foot Lifting	0.49±.05	0.39±.06	0.39±.06	0.32±.06	0.41±.06	0.45±.05

mately be compared with "practiced reaction-times," the age results obtained here may be compared within themselves and with others similarly obtained.

Correlations. The intercorrelations between the different reaction and coördination measures on the 100 adults are given in Table II. Between age and the time values there is in all six instances a positive and probably a reliable correlation which ranges from 0.25 to 0.55. Contrary to Wundt's remarkable individual showing and more in conformity with what is generally observed, these correlations indicate that with increasing age there is a decrease in speed of reaction and of coördination. This relationship is marked between age and speed of transferring the pencil with the right or dominant hand. Here the correlation is 0.55 ± .05, which is the highest value shown by 21 correlations. The correlation of age with the performance of the left or subordinate hand is less than that with the right hand,

[20]The tests were given in the order 1 to 5 indicated in Table I, and were repeated 10, 3, 5, 5 and 10 times respectively, hence the total number of all kinds of trials was not large (about 50 including the 10 "pursuit-reactions" which are not reported here). The subjects reacted to the sound of the clock which was also in full view.

although perhaps the significance of this difference may be questioned. This difference between the two hands may be from the fact that the left is more clumsy and consequently a larger chance factor enters. The correlation of age with the foot reaction is 0.49 ± .05. This is the largest correlation that age shows with reactive capacity as measured in this battery of tests. Adults as they grow older evidently suffer increasing loss in the prompt control of the foot but more consistently maintain their control of the hand for small movements.

The finger lift coördination on the average correlates with the other reactions and coördinations 0.40, and shows its highest correlation with the coördination of the left hand in shifting the pencil, 0.51 ± .05. Reaction by lifting the finger shows the lowest general correlation mean, 0.35. The two finger reactions correlate higher with each other (0.50), than either one does with anything else. Substantially the same state of affairs is found for the two coördinations, left hand and right hand, which with each other show 0.53 ± .05.

Age. The sample of data presented is hardly large enough to warrant division into age groups by decades; however, this has been tried and the classified means are presented in Table III. The averages have all been rounded into units of 0.01 sec. There is little change shown by the six age groupings which cover the range from 25 to 59 years. It is true that if we combine the means for the ages 25-29 and 30-34 yr. and compare these with the combined means for groups 45-49 and 50-54 yr., we find that in 4 of the 6 measures the younger adults are the faster, but the amounts of change seem slight. A much more regular and pronounced tendency is indicated by the last three groups. Here the mean values tend consistently to be above the average for the six younger groups and the retardation is progressive with increasing age. But the slowing produced by age does not result in means that fall beyond what is considered normal range for these several measures. The adults 60-85 yr. old are usually not more than approximately 25% slower than the comparable individuals in middle life. There were 5 women and 7 men who were older than 74 years (average age 79+). The reaction and coördination data secured on these 12 old people are presented in Table IV. For the finger lift the mean time is found to be 0.210 sec., which is 0.051 sec. (32% or almost one standard deviation) longer than the mean for the entire 100 subjects. Three of the 12 (all women) show values as short or shorter than the general mean of 0.159 sec. Furthermore, the slowest performance in the complete range (0.367,

Table I) was not made by one of the 12 oldest. The 9 *Ss* who were slower than the general mean are found to average 0.233 sec., which is 47% (1.32 S.D.) above the general mean. For the pencil shift with the right hand these 12 *Ss* average 1.54 sec., which is 27% (1.4 S.D.) increase above the general average of 1.21 sec. Here the slowest for the entire group of 100 *Ss* (M-3, 210 sec.) was among the 12, but there were 3 of the 12 who were faster than the general mean. In shifting the pencil with the left hand another of the 12 oldest *Ss*,

TABLE III

COÖRDINATION AND REACTION-TIME AVERAGES FOR 100 ADULTS CLASSIFIED ACCORDING TO AGE DECADE

(Time in sec.)

		Measure									
		(1)	(2)			(3)		(4)		(5)	
Age Group	No. *Ss*	Finger Coörd.	Rt. Hand Coörd.		Left Hand Coörd.	Finger Pressing		Finger Lifting		Foot Lifting	
		Ave.	m.v.	Ave.	m.v.	Ave.	m.v.	Ave.	m.v.	Ave.	m.v.
25-29	9	0.14	.02	1.12	.13	0.23	.03	0.21	.03	0.22	.01
30-34	11	0.13	.03	1.12	.09	1.26	.21	0.24	.08	0.22	.03	0.22	.03
35-39	13	0.15	.04	1.08	.08	1.28	.12	0.23	.04	0.21	.02	0.22	.02
40-44	15	0.15	.03	1.14	.08	1.31	.17	0.21	.03	0.21	.03	0.22	.03
45-49	17	0.17	.05	1.22	.14	1.33	.13	0.23	.04	0.23	.04	0.25	.04
50-59	9	0.13	.04	1.11	.10	1.26	.11	0.20	.03	0.22	.03	0.24	.07
60-69	11	0.18	.05	1.24	.22	1.37	.19	0.28	.11	0.23	.03	0.26	.04
70-79	11	0.20	.05	1.51	.26	1.55	.16	0.30	.08	0.26	.08	0.27	.05
80-89	4	0.20	.04	1.63	.16	1.81	.24	0.28	.03	0.28	.05	0.30	.03

(M-2), was the slowest in the 100 adults, and none of the 12 oldest reached or exceeded the general mean. Their average, 1.69 sec., is 25% (1.37 S.D.) above it.

As with the coördination results so with the reaction-time values, the 12 oldest *Ss* are usually distributed on both sides of the general group mean but are predominately among the slower than average. For reaction by pressing the finger the 12 oldest register a mean of 0.310 sec., which is about 29% (in this case only 0.72 S.D.) above the general average of 0.241 sec. Three of the 12 oldest were as quick or quicker than the average for the 100 adults. The 9 who were slower than the general average gave a mean of 0.335 sec. In reactions by lifting the finger the 12 oldest *Ss* included the S (M-3), who was the slowest, (0.450 sec.), of the entire 100, and also 5 others who were faster than the general mean. The average for these 5 fast old reactors is 0.195 sec. compared to 0.227 in Table I, *i.e.* 40%, or .62 S.D. faster. The average for the entire 12 is 0.280 sec., an increase of 23% or 1.0 S.D. Finally the reactions by lifting the ball of the right foot

average 0.292 sec. for the 12 oldest *Ss* in comparison with 0.242 for the entire group. This increase is equal to 1.06 S.D. or 21%. Again we find that 3 of the 12 oldest *Ss* reached or surpassed in speed the general group average. The significance of the differences between the means is not presented here because the number of cases is so small.

TABLE IV

INDIVIDUAL RESULTS FOR 12 ADULTS ALL OLDER THAN 74 YEARS OF AGE

Italic entries are as short or shorter than the comparable general averages for the entire 100 adults and boldface entries are in every case the longest values found in the group of 100 *Ss*.

(Time in sec.)

		Measure					
		1	2		3	4	5
Subject	Age (yr.)	Finger Coörd.	Rt. Hand Coörd.	Left Hand Coörd.	Finger Pressing	Finger Lifting	Rt. Foot Lifting
F-1	75	*.119*	*1.18*	1.47	.300	*.203*	.284
F-2	79	*.159*	1.53	1.82	*.240*	*.212*	.261
F-3	77	.294	*1.07*	1.42	*.226*	.238	*.206*
F-4	76	.206	1.59	1.52	*.228*	*.199*	*.226*
F-5	87	*.134*	1.61	1.68	.330	.370	.354
M-1	76	.221	*1.18*	1.58	.260	*.177*	.242
M-2	81	.232	1.86	**2.25**	.260	.282	.314
M-3	75	.164	**2.10**	1.69	.503	**.450**	.321
M-4	79	.277	1.57	1.61	.333	.332	.354
M-5	86	.211	1.73	1.46	.259	.277	.281
M-6	82	.233	1.36	1.86	.272	*.185*	.260
M-7	78	.263	1.71	1.92	.500	.436	.398
Ave.	79	.210	1.54	1.69	.310	.280	.292

In the general picture presented by Table IV we find no old person as slow as Exner's pauper, but in 3 instances out of the 6 possible the 12 oldest members includes the person who was the slowest of the 100 for the measure in question. In all 6 measures the group means for the 12 oldest persons are 20–30%, or 0.7 to 1.4 S.D. slower than the general mean, which again is confirmation of the correlation results previously described. Notwithstanding the general trend, that reaction-time and coördination speed become slower with increasing age, approximately 25% (3 out of 12) of our oldest subjects were as fast or faster than the average for the 100 adults from which they were drawn. This emphasizes the rather wide individual differences existing among the very old as among younger persons. In the 72 individual scores for reaction and coördination speed shown in Table IV there are 17 (24%) that are underlined; these 'fast responses' are values which equal or are speedier than the comparable group means for the 100 adults. It is noteworthy that they cluster:

15 of them are distributed 3 apiece to 5 *Ss*. This is indicative of the tendency to self-consistency that was shown by the intercorrelations presented in Table II. The 'fast responses' are very unequally divided between the sexes for there are 4 women with 3 as compared to one man with 3, and the 5 women total 13 while the 7 men achieve only 4. All 3 'slow reactions' fall to men. The material for this picture is too slight to be enlarged upon at present.

It is perhaps of special interest that 5 of the 12 oldest *Ss* reached or surpassed the general mean for reaction in lifting the right fore-finger while no one of the 12 reached the general mean for coördination with the left hand. These findings probably indicate a significant difference and seem to point to a higher degree of retention of right hand dexterity than of left hand facility in the very old. This may in part explain the reputed long retained skill of Wundt in reacting promptly.

Conclusion

It has been said that every man regards all other men as mortal except himself. It is probably equally true that every man regards himself as quick, or capable of acting quickly, and thinks of many, if not all others, as slow. Life and speed of action have thus their very personal subjective side, and in the absence of clear objective measurement are capable of great misjudgment. It is difficult for the old employee to be convinced that he is no longer able properly to manage the industrial machinery that has been in his charge. Often the employment director, who has the unpleasant task of making these readjustments and of counseling in favor of retirement, has little or no quantitative proof before him, and must think primarily in terms of a certain arbitrary number of years as the absolute age limit. Age is usually one of the important factors which define physiological and psychological efficiency. No machine can last forever; action necessarily means the reaction of wear and tear. But as the data presented in this paper indicate, there are distinct and measurable efficiency differences between individuals with chronological ages about seventy years, just as there are differences among people who are in their thirties or forties. The average for all who are older than seventy-four years, for example, may show a decrement of one-fourth or one-third from the general adult mean. But, on the other hand, one-fourth to one-third of these old people may still be as capable (in the functions tested) as the average adult, and to check them off just

because they happen to have been born seventy or more years ago may be as unfair to the individual as to drop him because he was born of foreign parents. Psychologists have convinced themselves and the educational world that there is such a thing as mental age, which, although it may ordinarily run a parallel course with chronological age, does not do so in all cases, and for this reason individuals must be considered on their individual merits, rather than arbitrarily lumped in calendar year units. Investigation such as is reported by the present sample of data indicates that calendar worship should be further curtailed as related to the problems and adjustments required in human life at its stage of later maturity. It is true, of course, that the social group must be protected. But it is equally obvious that the individual human being must not be neglected or too heartlessly sacrificed. Research must be extended until it is possible to interpret chronological age on the basis of capacity and capability indices which shall make possible a proper conservation of human life in its most mature phases.

Summary

(1) The general importance of reaction and coördination speed is considered, the relation of speed to age is traced, and the paucity of reaction-time and motor coördination data on older adults is noted.

(2) A rugged and inexpensive reaction-board is described which makes use of an electrical impulse counter (Klopsteg) that operates on 110 volt 60 cycle current. The instrument measures in 1/120 sec. and may be used outside the laboratory as conveniently as within it.

(3) The chronoscope in question serves as its own stimulus apparatus. The stimulus is usually auditory and resembles the sound made by a buzzer. The sound is produced by the armature of the clock when electrical impulses are being registered and continues from the moment the clock is started until it stops.

(4) A routine battery of reaction and coördination measurements that can be completed in about 15 min. is outlined. The reaction-movements recommended are finger pressure, finger lift, and lifting the foot as from the gas pedal of an automobile.

(5) In the measurement of coördination speed the *S* times himself, starting the clock by lifting his hand to the task, and stopping it when the task is finished. The reaction-unit provides equally well for the right and left-handed *S*s and is arranged to measure the skill of each hand separately.

(6) Data are given for 100 unselected adults ranging in age from 25 to 87 years. Simple reaction by pressing the finger required in this group an average time of 0.241 seconds; by lifting the finger, 0.227 sec.; and by lifting the ball of the right foot, 0.242 sec.

(7) The simplest extension and flexion of the fore-finger required an average of 0.159 sec. To reach a distance of 6 in., grasp a short, vertically placed pencil and thrust it into another hole required for the right or more dextral hand 1.21 sec., and for the left, 1.36 sec. The standard deviations are given for all means.

(8) The intercorrelations for age and the five measures reported are all positive and apparently statistically significant. They range from 0.25 (finger reaction by pressing) to 0.55 (pencil shift with the right hand).

(9) The different measures correlate positively among themselves. The range in coefficients is from 0.24 (right hand pencil shift with reaction by lifting the finger) to 0.53 (right hand with left hand pencil shift).

(10) The 12 oldest *S*s (average age 79 years) in the group of 100 tested registered means that were 20 to 30% slower than the comparable general means for the group as a whole. One-fourth of these oldest *S*s were, however, as quick or quicker than the average for the total group.

(11) The indications are that there is better retention of right or preferred hand dexterity than of left hand facility in the very old.

(12) In directing the education and activities of the young, psychologists consider mental age as well as chronological age. It is suggested that at the stage of later maturity chronological age should not wholly determine readjustment or retirement but be considered in the light of experimentally determined capacity.

MOVEMENT INTERPRETATIONS OF THE SILHOUETTE OF A REVOLVING FAN

By W. R. Miles, Stanford University

Somewhat earlier than Purkinje, Addam and Plateau contributed their observations on the illusion of reversed motion, phenomena such as these investigators noted seem to have been incidentally incorporated in various of the early lantern shows provided for public amusement. Nicholson[1] has described a public entertainment given by a Mr. Philipsthal at the Lyceum in London about 1802 in which these effects doubtless appeared. The lantern, instead of being on the same side as the spectators, was placed on the opposite side of the screen. No light appeared but what passed through the screen.

"His sliders are therefore perfectly opake, except that portion upon which the transparent figures are drawn, and the exhibition is thus conducted. . . . . All the lights of the small theater of exhibition were removed, except one hanging lamp, which could be drawn up so that its flame should be perfectly enveloped in a cylindrical chimney, or opake shade. In this gloomy and wavering light the curtain was drawn up, and presented to the spectator a cave or place exhibiting skeletons and other figures of terror, in relief, and painted on the sides or walls. After a short interval the lamp was drawn up, and the audience were in total darkness, succeeded by thunder and lightning; which last appearance was formed by the magic lanthorn upon a thin cloth or screen let down after the disappearance of the light, and consequently unknown to most of the spectators. These appearances were followed by figures of departed men, ghosts, skeletons, transmutations, etc., produced on the screen by the magic lanthorn on the other side, and moving their eyes, mouth, etc. by the well-known contrivance of two or more sliders . . . . The figures appeared without any surrounding circle of illumination and as many of the spectators had no previous view or knowledge of the screen or any visible objects for comparison, each was left to imagine the distance according to his respective fancy. After a short time of exhibiting the first figure, it was seen to contract gradually in all its dimensions, until it became extremely small and then vanished."

Nicholson makes no mention specifically of illusions of movement, for, as we can readily understand, in an entertainment of this sort no attempt at discrimination between the objective and the subjective would be made. To a naive observer each kind of movement is as real as the other. This is well illustrated by the behavior of Whipple's[2] student, who when he stopped the kymograph drum that had been revolving for some time in one direction, lifted off the contact arm so that the "backward movement of the drum" might not "damage the contact point or the paper."

*Accepted for publication September 5, 1930.

[1]William Nicholson, Narrative and explanation of the appearance of phantoms and other figures in the exhibition of the phantasmagoria.: With remarks on the philosophical use of common occurrences, *J. Nat. Philos. & Arts*, 1, 1802, 147-151.

[2]G. M. Whipple, The possibility of psychical factors in illusions of reversed motion, *Science*, 23, 1906, 507-509.

The windmill illusion. Among the equivocal illusion figures, the windmill is one of the most interesting, because in it foreground and background alternation is associated with successive reversals in the direction of movement. Sinsteden[3] was the first to report his observation that a windmill seen at an angle of 20°-30° from the plane of rotation and at some distance without too strong a light appears to revolve part of the time in one direction, and then suddenly to change its movement to a revolution in the opposite direction. The suggested explanations of this striking effect have been reviewed by Wallin.[4]

If the windmill is in connection with a building, or sizable machinery cage, then the two interpretations of the motion will make the mill appear to be situated now on the front side of the building, and again on the back. If, however, the support of the mill is a simple perpendicular affair, the observer may see the wheel always in the same relation to it but the angle with reference to him will shift as the mill appears to change the direction of its rotation. Usually for binocular vision the illusory phase of motion is more brief and infrequent than the true phase. Kenyon independently observed a similar illusion in connection with a two-winged pendant electric fan "such as is commonly seen in restaurants and shops."[5] Viewed from a distance of about 30 ft. when revolving at a moderate speed, a fan of this kind may appear to turn in either direction. Kenyon reports several interesting facts which he says "may be well known to everyone who has watched machinery." At times he saw the two vanes appear to flap together. There was another phase which he describes as a "continually withdrawing into and pushing out from the hanging rod," a movement which seemed to limit itself entirely to two dimensions and to be independent of depth perception.

The writer has pointed out that it is quite possible to get alternate revolution meaning from elliptical diagrams so drawn as to present reversible perspective,[6] and further he has described the common two-armed lawn sprinkler as probably the best natural device in which to see the illusion of alternate direction of rotation. The lawn sprinkler is usually viewed at a very slight angle from the plane of rotation. Furthermore, the plane of rotation and the eyes of the observer both tend to be in the horizontal position. Therefore, depth perception is minimized and the observer can more easily confuse foreground and background in reference to the whirling arms.

Until actually making observations one would think that the curved streams of water issuing from the sprinkler would interpret the motion correctly. But these streams are subject to the same equivocal interpretation of what is foreground and what is background as are the moving parts of the mechanism from which they issue.[7]

---

[3]W. J. Sinsteden, Über ein neues pseudoskopisches Bewegungsphänomen, *Poggendorff's Annalen*, III, 1860, 336-339.

[4]J. E. W. Wallin, *Optical Illusions of Reversible Perspective*, 1905, 21-24.

[5]F. C. Kenyon, A curious optical illusion connected with an electric fan, *Science*, 8, 1898, 371-372.

[6]W. R. Miles, Figure for the 'windmill illusion,' *J. Gener. Psychol.*, 2, 1929, 143-145.

[7]Under some conditions in place of seeing the water hurled from the sprinkler to the ground, it is possible to see the drops jump from the ground up along the curved path of the trajectory and disappear at the point where the reflected light from the drops markedly diminishes. This occurs in the drops on the other side of the sprinkler when one is looking down on it at an angle of about 15° and with the sun at the observer's back.

Apparatus

Various models have been constructed for demonstrating the windmill illusion.[8] Sinsteden, taking his observations to the experimental stage, noted that the illusion could be produced by viewing, obliquely, at a distance of about 25 ft., a cardboard cross made with arms about 12 in. long and ½ in. wide. Hering[9] constructed a windmill model from wire and observed it monocularly from an angle of 45°. McDougall has used a laboratory model for the windmill illusion probably more extensively than any other investigator, employing it as an attention test.[10] His model is made of four brass rods, each 20 cm. in length fixed at right angles to one another, and at one end of a long steel rod. The steel rod is mounted horizontally in firm bearings, and arranged for rotation by a motor-driven pulley placed at one end. The model is set up in a dark room so that the observer seated at a distance of 2 m. can see the four blackened brass rods and a portion of the steel axis. The rotation was in a plane a little oblique to the observer's sagittal plane, and seen against a bright background formed by a large sheet of paper illuminated from behind. The stimulus was thus mainly a silhouette made by interposing the model between the observer and the bright background. With no other light on the windmill the observer looking with one eye found it impossible to discover which was the actual direction of rotation—"whether the wheel is, as it were, rolling toward him or away from him." The 8 *Os*, whose responses are reported, were equally likely to perceive either kind of motion first; and after a period when the two motions had each been seen, they were found to alternate at approximately regular intervals. If the wheel was looked at with both eyes, subjects could detect the actual direction of rotation, but after prolonged observation with both eyes, the illusory phase would come occasionally for brief periods.

The writer undertook to arrange a model which would completely obscure from the subject any direct indication of the mechanism, and would be so free from depth cues as to permit the regular use of binocular vision. A small two-winged fan was fixed in a pendant position behind a milk-glass window, illuminated with a condensed filament electric lamp. The lamp was placed at such a distance behind both fan and window that the shadow of the fan would be shown on the window as a fairly sharp silhouette. The apparatus is illustrated in A of Fig. 1. In the cut the front panel carrying the milk-glass window, which measures 4¼ x 7½ in., has been removed, and placed at the right of the box. The open box shows the position of the fan and of the blower directly below it. Behind the shaft of the fan, and partly covered by it, is the vertical lamp bulb mounted in its base on a supporting horizontal rod of wood so that the lamp can be placed at different distances from the fan as desired. The lamp is a 200-w. projection Mazda to be used in vertical position, and is placed 28 in. behind the fan shaft. A single lamp cord for use with 110 A.C. or D.C. outlet serves both the blower and the lamp. The blower is a hairdryer unit without the heating element. The shaft of the fan is concentric with the orifice of the blower. The latter makes some

[8]The writer acknowledges the aid of Mr. R. H. Slocumb in arranging the apparatus.

[9]Wallin, *op. cit.*, 24.

[10]W. McDougall, Physiological factors of the attention-process, *Mind*, 15, 1906, 328-359.

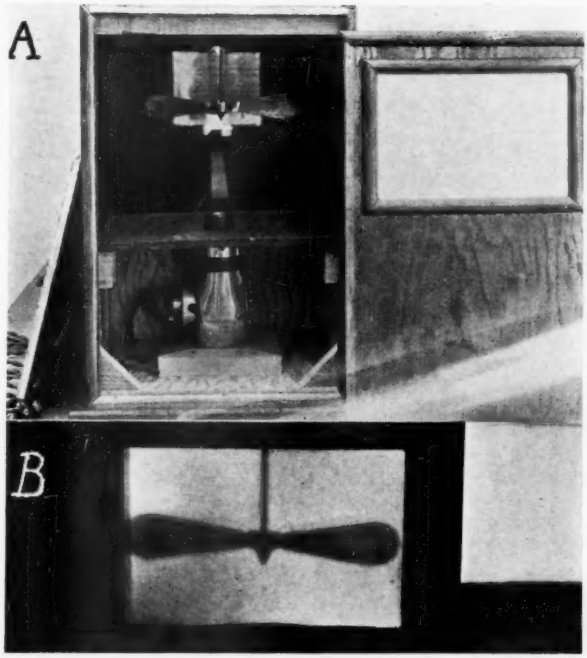


FIG. 1. MODEL FOR DEMONSTRATING THE WINDMILL ILLUSION
A, arrangement of fan, blower and lamp. B, silhouette of the fan as it appears to the observer.

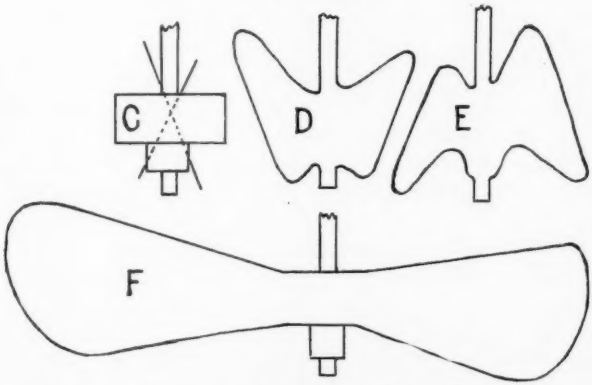


FIG. 2. DIAGRAMMATIC ASPECTS OF THE FAN SILHOUETTE

noise but this feature has the advantage of keeping the *Os'* attention on the box, as well as of mystifying them with reference to the nature of the mechanism. The rate of rotation varies somewhat about the mean speed of 80 r.p.m. This variation seems to offer no disadvantage.

When the fan is tangential to the lamp and to the *O*, the stimulus window gives an appearance as in B of Fig. 1. The slight double line at the left in this cut is due to the window frame shadow cast by the outside illumination. The tilting of the blades is non-observable in this silhouette. The tilt, however, shows in A of Fig. 1, and the reader will note that the fan, as shown, would revolve in a clockwise direction when viewed from the top, *i.e.*, the blade at the right in the picture will swing toward the front and from right to left. As the blades turn the tilt will be visible in the silhouette. The diagrams in Fig. 2 illustrate some of the positions. In C the blades are in line between the *O* and the point of illumination. They are at a slant of 25° from the vertical. It is also seen that one of them casts a longer image than the other, which is of course due to the magnification by reason of one blade being nearer the source of illumination. This maximum effect is in the ratio of 29 to 36. The blade nearest the lamp and farthest away from the *O* casts an image on the ground glass 36 mm. wide, while the other blade of the same dimensions casts an image 29 mm. wide. With rotations 10° one way or the other, we get D and E respectively, whereas F represents a 45° rotation, between C and B.

These various aspects of the stimulus are presented in rapidly succeeding and merging order, and are scarcely distinguishable as separate phases when the stimulus is viewed intently, with fairly steady fixation. When the window is viewed in the ordinary daylight illumination of the classroom the intensity is not such as to give troublesome after-images, and the *Os* can continue their observations for sufficient periods without tiring their eyes.

Experiment I

Procedure. Without any explanation whatsoever about the stimulus material the box window was suddenly exhibited with the fan *in full motion* to 27 college students, seated as directly in front of it as possible. The *Os* were naïve in the sense that the windmill illusion or other related figures had not been presented to them. The students were then asked to make short written descriptions of the stimulus material and were requested to put down in a column from top to bottom on large sheets of paper provided any changes which occurred.

Results. The results of this experiment are indicated in Table I.[11] At the left, are given the designation number of every *O* and the sex. The next column shows the total number of changes or different motion patterns that the individual experienced within the 10 min.

[11]The writer is obliged to Mr. Albert Walton for assistance in securing the data reported.

of uninterrupted observation. These results will be understood by reference to the other columns of the table. The column headed "First responses" turns out to be always L or R. R signifies revolving to the right; that is, the end of the paddle seen next the sub-

TABLE I

RANGE AND SUCCESSION OF MOVEMENT INTERPRETATIONS

(L = left; R = right; NS = not specified; F = front; B = back)

O	Total no. distinct patterns	First responses	Order of patterns												
			Revolution		Stretching	Clapping					Oscillating	Crossing		Miscellaneous
			L	R		NS	F	B	L	R		F	B	
1 M	4	L	1	2	3									4—up and down, fluttering
2 M	6	L	1	2		3			5	6		4		
3 M	6	L	1	3	2		4	5						6—fluttering
4 M	4	L	1	2	3						4			
5 M	4	L	1	2	3						4			
6 M	3	L	1	2	3									
7 M	4	R	2	1		4					3			
8 M	3	L	1	2	3									
9 M	4	R	2	1	4	3								
10 M	4	R	2	1	3						4			
11 M	4	L	1	2	4	3								
12 M	4	L	1	2	3		4							
13 M	4	R	2	1	4	3								
14 F	4	L	1	2			4	3						
15 F	3	R	2	1	3									
16 F	5	L	1	3		4					5			2—fluttering
17 F	3	L	1	2	3									
18 F	3	R	2	1	3		4							5—revolving and stretching; 6—half up and down
19 F	6	L	1	2	4	3								
20 F	4	L	1	2	4		3							
21 F	3	R		1	3	2								
22 F	3	R	2	1	3									
23 F	4	L	1	2	3	4								
24 F	7	L	1	2	4		5	6			3		7	5—revolving and stretching
25 F	8	R	2	1	6		4	7			3	8		
26 F	5	L	1		3		2	5			4			
27 F	3	R	2	1		3								

ject is passing from left to right, which would be counter-clockwise looked at from above. L is revolving to the left, or clockwise, and is the actual revolution direction of the fan. After the *O*s had entered their preliminary descriptions and a full minute of observation had elapsed, *E* explained to them how they should describe the direction of motion, provided they had seen the stimulus as a revolving some-

thing turning in the horizontal. The matter of the correct description of the revolution direction was stressed, and the writer is confident of the results. In the group 17 of the 27 *O*s saw the stimulus L, with the near side passing from right to left. The ratio of correct to illusory interpretation is therefore 1.7 to 1 initially. The first response whether or not corresponding to the actual direction of the mechanical stimulus was always a response of "revolving," and in the majority of cases in this group even the direction was correctly interpreted. Later in the 10-min. period of observation the *O*s had lost the basis for the first impression, and could form no firm conviction as to the actual direction of revolution, R seeming as natural and as satisfactory as L.

Other movement patterns were designated as "stretching," "clapping," "crossing," and "oscillating," named in order of frequency as indicated in Table I. "Stretching" and "clapping" seem to be convenient names for the types of observed movement mentioned by Kenyon. We notice that after revolving, "stretching" is the second most natural interpretation. Here the stimulus expands from a form like C in Fig. 2 to that shown in B of Fig. 1. Apparently it is pulled out or bulges out from the center, elongating and swelling and then going through a complementary contraction and retraction. Fourteen of the *O*s got stretching as their third effect in order of sequence. One got it as the second effect. With 6 it was in the fourth position and only one of the 27 got it as late as sixth in order. "Clapping" usually came as the third or fourth interpretation. It was said to occur to the front, to the back, to the right, or to the left, apparently according as the *O* thought of the position of the axis of the two arms. The *O*s who analyzed the clapping most frequently spoke of it as taking place in front. They said that the arms from being extended straight to the side were brought together in front, then separated, returning to the original positions on the sides.

A motion somewhat like that of clapping, but which appears to be at half the speed is what we may call "crossing." Here one arm comes from the right and passes around a half-revolution to the left, synchronously the arm that was at the left passes around in the opposite direction to the right, as an individual might swing his arms before causing them to pass each other. The very striking effect can be held practically as long as any other, but it appears that the naïve *O* is much less likely to hit on it as an interpretation of the stimulus than he is to notice clapping or the motion designated as "oscil-

lating." In the latter, the two blades remain in line as a fixed propeller, but in place of turning entirely around in one direction, they seem to start a revolution, turn a short way, about 30°, and then reverse and turn back. They oscillate, as it were, like a weathervane in the wind. This was noticed by 8 of the 27 *O*s, and by them was seized on as third, fourth or fifth interpretation.

TABLE II

OBSERVED MOVEMENT INTERPRETATIONS IN ORDER OF FREQUENCY

Pattern	No. Cases	Total No.	Percent
Revolution		27	100
L	26		
R	26		
Stretching	22	22	81
Clapping		18	66
Not specified	10		
Front	8		
Back	5		
Left	1		
Right	1		
Oscillating		8	30
Perpendicular	8		
Tangent			
Diagonal			
Slanting			
Crossing		3	11
Front	2		
Back	1		
Miscellaneous		5	19
Fluttering	3		
Revolving and stretching	2		
Shaft motion	1		

There were a few miscellaneous interpretations that were described as "fluttering," "like a bird," "bird wings," or "alternate up-and-down motions,"—interpretations which may have been largely a response to the slant of the wings. There were 2 *O*s who spontaneously reported a combination of revolving and stretching. One arm was seen to revolve continuously, while the other arm extended after the revolving arm had passed its position, and retracted as it came around again.[12] The clapping, crossing, and oscillating, can apparently be in any axis, or position, with the assumed circle of motion, but to see the motion occurring directly in front of the supposed axis is much the more common and also the phase that is more permanent.

[12]Flapping bird wings have been seen as a screw propeller turning about the bird's body, S. P. Thompson quoted by A. H. Pierce, The windmill illusion, *Science*, 8, 1898, 479-480.

TABLE III

PERSISTENCY OF INTERPRETATION AND RESPONSE TO SUGGESTED PATTERNS

Y = yes; N = No

O		Most persistent kind of direction	Names	Suggested pattern of motion						
				1 Revolution L	2 Revolution R	3 Stretching	4 Clapping F	5 Clapping B	6 Oscillating	7 Crossing
1	M	—	—	Y	Y	Y	Y	N	Y	N
2	M	Revolution R	hands	Y	Y	Y	Y	Y	Y	Y
3	M	—	wings	Y	Y	Y	Y	Y	Y	Y
4	M	Revolution R	blades	Y	Y	Y	Y	Y	Y	Y
5	M	Revolution	—	Y	Y	Y	Y	Y	Y	Y
6	M	Revolution	paddles	Y	Y	Y	Y	Y	Y	N
7	M	Revolution R	wings	Y	Y	Y	Y	Y	Y	N
8	M	Revolution R	—	Y	Y	Y	Y	Y	Y	Y
9	M	Clapping	—	Y	Y	Y	Y	Y	Y	Y
10	M	Revolution	—	N	N	N	N	N	Y	N
11	M	Revolution L	hands dumbbells	Y	Y	Y	Y	Y	Y	Y
12	M	Revolution	wings	Y	Y	Y	Y	Y	N	Y
13	M	—	wings	Y	Y	Y	Y	Y	Y	N
14	F	Revolution R	wings	Y	Y	Y	Y	Y	N	Y
15	F	Revolution	—	Y	Y	Y	Y	Y	N	N
16	F	Clapping	wings	Y	Y	Y	Y	Y	—	—
17	F	Revolution L	—	Y	Y	Y	Y	Y	Y	Y
18	F	Stretching	—	Y	Y	Y	Y	Y	Y	Y
19	F	Stretching	—	Y	Y	Y	Y	Y	Y	N
20	F	—	wings	Y	Y	Y	Y	Y	Y	Y
21	F	—	blades	Y	Y	Y	Y	Y	Y	Y
22	F	Revolution R	—	Y	N	Y	Y	Y	Y	Y
23	F	—	—	Y	Y	Y	Y	Y	Y	Y
24	F	Clapping	paddles	Y	Y	Y	Y	Y	Y	Y
25	F	—	paddles	Y	Y	Y	Y	Y	Y	Y
26	F	Stretching	points	Y	Y	Y	Y	Y	Y	Y
27	F	Revolution R	—	Y	Y	Y	Y	Y	Y	Y

The *O*s averaged 4.25 different patterns of movements. There is no significant difference shown between men and women in respect to number. The women average about 4.3 and the men 4.1. In Table II a tabulation is given of the frequency of the different kinds of interpretations of motion. All the *O*s got the interpretation of revolving, but it happened that one of the 27 got revolving to the right, and not to the left, while one other got revolving to the left but not to the right. Twenty-two *O*s, 81%, got the effect of stretching. There were 18 *O*s who got the effect of clapping, 8 of these designated it as in front; 10 used the term "clapping," or an equivalent without stating the axis, and 5 referred to clapping at the back. The total amounted to 66% of the group. For oscillating, crossing, and miscellaneous, there were 30%, 11%, and 19%, respectively.

The responses of the different *O*s were examined as to the most persistent phase or kind of motion indicated by their records (see Table III). The instruction had been given to make note of the varieties of happenings which occurred, but most of the *O*s put down successive phases in the order of their appearance, regardless of whether they had previously observed and reported them. In some cases it could not be decided that the individual showed a persistent phase. Of the 20 *O*s who could be so judged, it is found that 14 (9 men and 5 women) show revolution R or L, as the dominant pattern. This amounts to 70% of the group who showed a persistent trend. There were 3 *O*s, all women, who more frequently mentioned stretching than any other kind of interpretation, and also 3 (2 women and 1 man) who reported clapping as the most persistent. Considering that 17 of the *O*s gave their initial impression of the stimulus as revolving to the left, it is indicative of the later confusion of cues that the persistency of the rotation turns out to be 7 R to 2 L.

Different terms were used in naming the moving parts of the observed object. Those occurring most frequently are, in order: "wings," "paddles," "blades," "hands," "points," and "hands with dumbbells." Twelve *O*s did not use any term or name for the moving parts. The naming was in any case wholly incidental, as the *O*s were entirely uninstructed with reference to it.

Experiment II

Procedure. At the end of the 10 min. period of observation just described, a second part of the experiment was initiated, which lasted for a period of about 5 min. In this experiment, various movement

interpretations were suggested, and within 15 sec. after the suggestion was made, the individuals were asked to reply "yes" or "no" on whether or not they could *now clearly see* this motion taking place in the stimulus-object. The motion suggestions given were in order as tabulated from left to right in Table III.

Results. As shown in Table III, the replies were predominantly "yes" (Y). One *O* (No. 10, a man) was regularly unable to get the pattern of motion suggested within the time allowed; another (No. 22, a woman) was unable to get revolving R; otherwise all the *O*s were able to see revolving left, right, stretching, and clapping in front, in the order named and within the time allowed. *O* No. 1 could not get clapping behind. There were 3 who failed to get the effect of oscillating, and 7 who failed to get the interpretation of crossing movements.

These experiments show that the type of stimulus-object used lends itself remarkably well (a) to a wide diversity of interpretation in which any one of many patterns is as real and as satisfactory as any other, and (b) to a very ready induction into a variety of interpretations through the suggestion (verbal or gesture) of some other *O*.

Discussion

The windmill as a subject for optical illusion has been typically described as revolving in one direction or the other. Ordinarily, the stimulus is identified for what it is, whether seen near or far away. Familiarity with it is so common that any windmill can scarcely escape the interpretation of a wheel in motion, revolving in one plane about its axis. Then, too, a windmill, having several fans, even though it has no objective phenomenological circumference, still closely resembles a true wheel. Therefore, the effect it gives of rotary motion is not displaced by that of stretching or clapping, or by oscillating or crossing—movement interpretations frequently reported in our experiments. The effects other than the revolving are dependent upon having a mechanical stimulus that is more fragmentary than an entire windmill. Kenyon saw some of these non-revolution illusions in the two-blade fan suspended from the ceiling, and the writer has discussed them previously in connection with the simple two-armed lawn sprinkler.[13] The fragmentary mechanical object when revolved presents more distinct visual aspects (see Fig. 2). Our 2-bladed mill is a much more complex stimulus for visual

[13]Miles, *op. cit.*

perception than are the 4 blades or rods as used by McDougall, or the 10 or 12 blades found in the ordinary windmill wheel. The mechanically simpler but perceptually more diversified stimulus permits greater latitude in satisfactory interpretation. But all the recorded interpretations were variations of circular or pendular motion and in this were perfectly faithful to the objective machine.

After having discovered four or five different motion interpretations which fit well, and each of which has persisted for a few moments so that it could be clearly made out and verbalized in description, an *O* can voluntarily entertain any one about as well as any other. There is always a tendency to keep shifting from one interpretation to another and *O* can, to some extent, determine what he will see. Ordinarily, however, he does not choose and if he does attempt to see a particular pattern he finds that he is not instantly in control. Some shifts are easy enough to make. Perhaps the easiest is, from clapping in front, to clapping behind. It is much more difficult to 'require the stimulus' to clap to the right in place of in front. After maintaining the interpretation of clapping for some moments, it is also rather difficult to organize the interpretations of crossing. One factor which inhibits this is that crossing has a much slower apparent rate.

Every interpretation tends to be a complete one, and to involve the entire stimulus material. It is generally all caught and used in the movement meaning in an appropriate way. A possible exception is the case of *O* 19, who reported the combination of revolving and stretching. Half and half, or partial interpretations of our windmill stimulus material, are rare, although they are not infrequent with the reversible figures such as the block diagram.

None of the *O*s saw a movement interpretation which he cannot describe in fairly simple words; or perhaps we should say, he sees only those for which he can readily formulate suitable mechanical equivalents.[14] It seems quite likely that one of the reasons why crossing is so infrequent as a movement interpretation rests on the fact that a mechanism cannot be thought of that can bring its arms together at the same level, then pass them through each other going on to the original outstretched position but with arms exchanged. *O*s illustrated the movement by swinging their arms and passing one above the other, but they state quite distinctly that the arms of the stimulus

[14]This we may infer from the written reports. *O*s always mention something specific and never that there were any things seen which they could not put into words.

pass through each other as they see them. Oscillating in a position tangential to the observer was not reported by any of the 27 *O*s, and none of them saw the figure oscillating in a position diagonal to their view. Of course, if the fan is truly horizontal and tangential it could scarcely be seen to be oscillating. The initiated can, however, cause it to oscillate in the diagonal. Probably when *O*s are available with more elaborate concepts of mechanical actions and models new and equally satisfying interpretations will be found.[15]

It is noteworthy that stretching occurs so frequently and so early (third position) in the series. "Rubber" is frequently mentioned in the descriptions but no clear statement is made as to what produces the stretching. Stretching is an interpretation which involves motion in only two dimensions, the depth factor having been omitted. Lack of sharp convergence and accommodation tend undoubtedly to induce this meaning phase and may account for the frequency of this interpretation.

The different interpretations cannot be induced instantly; they ordinarily require 5 or 10 sec. to become established. They are not as promptly under control as are the foreground-background relations of a complicated figure, or the usual figures for reversible perspective. It seems definitely possible to induce certain phases by pursuit movements of the eyes. For example, the writer finds it easier to induce the crossing pattern by executing pursuit eye-movements than by gazing at or beyond the stimulus figure. When a given phase of interpretation is in progress, steady fixation at some position on the window, seems to favor holding or prolonging that phase. It is obvious, however, that the particular interpretation does not depend so much on one exact fixation position on the window, as in the case of the usual static figures. Eye-movements, in so far as they serve to initiate new interpretations, probably do so in part by creating a new distribution of the stimulus-material. It is probable that pursuit movements are of considerable importance in exploring and analyzing our visual stimulus. They doubtless help at times to give an initial impulse to certain interpretations, but their occurrence, unless of an exceedingly regular nature, is a disadvantage to the maintenance of any one interpretation.

[15]No subject reported the wings as rolling up and then unrolling which presumably might have been seen.

The influence of central process and of logical analysis is evident in some of the results. If an *O* sees the stimulus clapping and thinks "It is clapping in front," his thinking, "Can it also clap behind?" will probably be sufficient reason for his seeing this activity. If, however, this question fails to arise in his mind, he is unlikely to have an alternate to the clapping in front. If he continues his logical survey, and asks, "Can it clap to the left?" he may see it do so, but this movement is less likely to be caught than the other for the reason that the entire space between the central axis and the right hand periphery is not wholly and consecutively unoccupied. The arms sweep through the space, and so the *O*'s stimulus will not clap as naturally to the left as in front or in the back. The experiments have shown that the most spontaneous meaning for the observed movement is that of revolving. This and the other less frequent but none the less characteristic interpretations appear to follow the order of mass or frequency of experience.

Summary

(1) An apparatus for demonstrating the windmill illusion is described. This consists of a small two-blade fan pendant behind a milk-glass window and illuminated in such a way that the silhouette of the fan in motion is cast upon the window. The fan is made to turn at the rate of 80 revolutions per minute.

(2) The silhouette stimulus when presented suddenly with the fan in full revolution and without introductory description or explanation called forth the spontaneous interpretation of revolving. About two-thirds of the *O*s interpreted the silhouette as revolving in the way the mechanical fan was actually turning. However, after the experience of seeing it reverse and go in the other direction (illusion), they became unable, during the rest of the period of 10 min. of observation, to certainly decide which was the true direction of revolution.

(3) A fragmentary fan of the kind described constitutes a more complicated perceptual stimulus than does a four-winged fan or a complete windmill. The two-winged fan seen in silhouette lends itself to the interpretations of "revolving," "stretching," "clapping," "oscillating," and "crossing," as well as miscellaneous other interpretations. These temporarily occur approximately in the rank order named.

(4) Apparently *O*s find only those movement interpretations for which they have concepts which serve as mechanical equivalents, and of which they are able to give fairly adequate verbal descriptions. The variety may be much extended according to the experience of the *O* and his range of pertinent mechanical concepts.

(5) All of the different interpretations are equally adequate, after the *O* has been initiated in them, and although they will not spontaneously persist for equal periods of time, they can be maintained about equally well.

(6) Pursuit eye-movements may aid the shift from one interpretation to another in some cases, but fairly steady fixation is a more suitable condition for maintaining any one interpretation.

(7) Stretching, a distinctly two-dimensional interpretation, is probably favored by relaxed convergence and accommodation and by indirect vision.

(8) Different interpretations are rather promptly accepted by most *O*s upon suggestion.

STUDIES IN AFFECTIVE PSYCHOLOGY

By Emily H. Kniep, Winona L. Morgan, and Paul Thomas Young,
University of Illinois

XI. Individual Differences in Affective Reaction to Odors

The present study is a continuation of an attempt to develop the technique for testing affective reactions.[1] The general method is to keep experimental conditions constant and to observe individual differences in the tendency to be pleased and displeased by chemically pure organic substances.

Experimental conditions. The substances used in this study were selected after careful consideration. All had to be chemically stable at least for several years. Some substances were rejected because of their high diffusibility; some because they stimulated the tactual nerves of *S*'s nose; others because they tended to gum the bottles. The following list was finally selected:

(1) Camphor, 13 grm.
(2) Methyl salicylate, 26 grm.
(3) Vanillin, 11 grm.
(4) p-Dichlorobenzene, 16 grm.
(5) Menthol, 13 grm.
(6) Phenol, 18 grm.
(7) Acetophenone, 21 grm.
(8) Nitrobenzene, 20 grm.
(9) Geraniol, 27 grm.
(10) Ethyl cinnamate, 28 grm.
(11) n-Caproic acid, 17 grm.
(12) Quinoline, 18 grm.
(13) Heptyl aldehyde, 16 grm.
(14) o-Bromotoluene, 29 grm.
(15) Di-Phenyl ether, 23 grm.
(16) Empty bottle, control.

The list shows the numerical designation of the substance, its chemical name, and approximate number of grams used. Substances were kept in wide-mouthed amber bottles having a capacity of 65 c.c. Prior to the experiment the bottles were thoroughly cleaned and the numbers were scratched with a diamond point on both bottle and stopper. Greatest care was taken throughout the experiment to keep bottles free from contamination. The control bottle, No. 16, was empty and sterilized but to the eye it resembled the other bottles.

The experiment was conducted in a black-walled room of the laboratory. Illumination was kept constant by an ordinary 50-w. tungsten lamp; glare was eliminated. Bottles of chemicals were placed on a table behind *S*. This table was located at the bottom of a vertical air shaft which extended to the floor above. Two fans, one beneath the table and one at the top of the shaft, kept a current of air moving away from the experimental room, thus freeing it from stray odors.

S was asked to read the following instruction:

*Accepted for publication September 3, 1930.

[1]For the first report see P. T. Young, The scale of values method, this Journal, 42, 1930, 17-27. The present work was done between Nov. 11, 1929 and April 1, 1930.

Be passive and composed throughout the experimental period. Odors will be presented. Close the eyes during the time of smelling and reporting. Report upon the following. (1) Do you like the odor or dislike it? (2) How much do you like (or dislike) it? Please indicate by such words as : *very* pleasing or displeasing; *moderately* pleasing or displeasing; *weakly* pleasing or displeasing; *indifferent*. (3) Report anything further which you consider significant.

Stimuli were presented in an order determined by chance with an interval of approximately 50 sec. between successive presentations. *E* watched the respiration of *S* and presented the odor at the beginning of inspiration. The bottle was held under the nose for a period of 3-4 sec. at a distance of about 2 cm. A 'ready' signal was given 1½ to 2 sec. before the presentation. A complete record of *S*'s report was kept on mimeographed sheets prepared for that purpose. Time between the first inspiration and verbal report was recorded on a stop-watch without *S*'s knowledge.

The total experiment includes work with 200 *S*s; 100 in Part A, and 100 in Part B. The *S*s were students at the University of Illinois between the ages of 18 and 24 yr. Equal numbers of men and women were used.

A. Affective Value of the Odors

The percentage of pleasantness of an odor was computed by the following formula:[2]

$$\text{Percent P} = 100\,(p + i/2)/(p + i + u),$$

in which p equals the total number of reports of pleasant, i the number of indifferent, and u the number of unpleasant.

Inasmuch as there were 100 *S*s, 50 men and 50 women, the percent P for an odor is the number of *S*s reporting it as pleasant, plus half those reporting it as indifferent. The percent P for every odor is given in the following list.

Serial Order	Odor No.	% P	Serial Order	Odor No.	% P
1	9	81	9	4	83
2	12	15	10	2	90
3	7	56	11	5	62
4	14	20	12	13	7
5	10	31	13	1	70
6	6	31	14	3	93
7	16	61	15	8	15
8	11	9	16	15	44

In selecting the odors we aimed to divide the intensive distance between very U and very P into approximately equal steps.[3] The success in this is shown in Fig. 1, in which the 16 stimulus-objects are ranked from U to P according to percent P. These percentages

[2]The formula has been taken from J. G. Beebe-Center's paper upon the law of affective equilibrium, this Journal, 41, 1929, 56.

[3]On the basis of previous experimental work we had selected Nos. 3, 2, 4, 9, 1 as probably pleasing; Nos. 11, 12, 14, 10 as probably displeasing; Nos. 16, 5, 7, 6 as approximately indifferent. Three new and unknown odors were added which we hoped would be displeasing—Nos. 13, 8, 15.

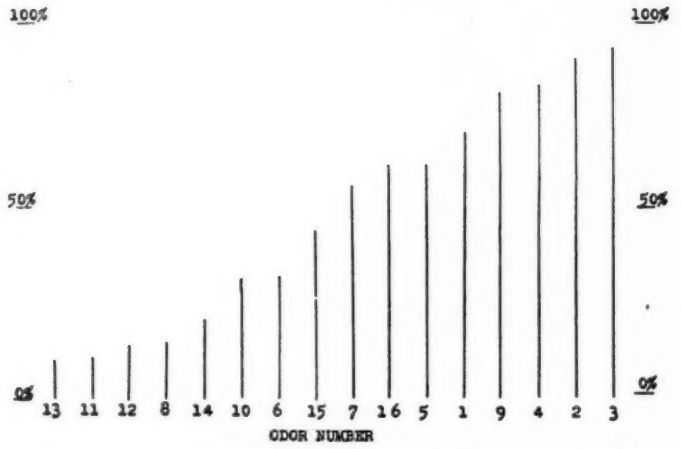


FIG. 1. PERCENTAGE OF PLEASANTNESS FOR THE VARIOUS ODORS USED

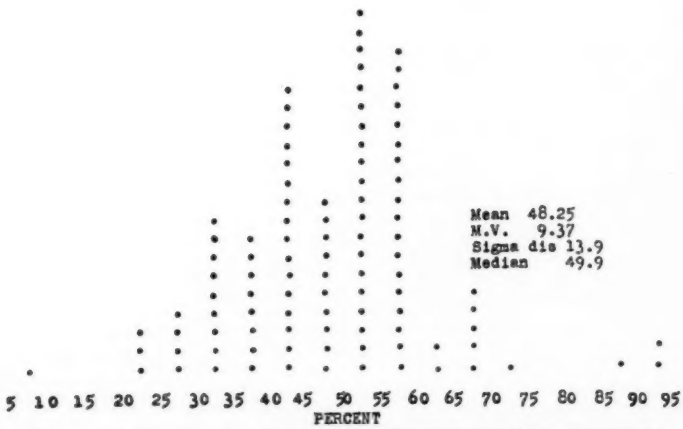


FIG. 2. DISTRIBUTION OF RAW SCORES ACCORDING TO PERCENTAGE OF PLEASANTNESS

indicate general affective values which do not depend upon any particular group of *Ss*.[4] The series gives approximately a linear distribution.

Affective reactions. How readily is a particular *S* pleased or displeased by the total group of odors? If an *S* reports that all 16 stimulus-objects are pleasing, he is readily pleased. If, on the other hand, he finds none of them pleasing, he is 'hard to please' by the odors and readily displeased. Most *Ss* fall between the two extremes.

A percent P has been worked out for every *S*. One hundred percent (100%) would indicate that all odors are reported pleasant and that none of them are reported unpleasant. Zero percent (0%) would indicate that none of the odors are reported pleasant and that all of them are reported unpleasant. Fifty percent would indicate that half are P and half are U, or else that all are I.[5]

Fig. 2 shows the distribution of 100 *Ss* according to the percent P. The shape of the curve definitely resembles the normal curve of probability. A curve for weighted scores (not shown here) gives an even closer approximation to the normal curve of distribution.[6]

Discussion. Considered statistically, one might argue that the normal curve should be expected on the basis of chance. If, for example, instead of 16 bottles and the report of P or U we had tossed coins and recorded heads or tails, the total distribution for the number of heads (or tails) would be normal. Does the shape of our curve depend upon chance variation of non-affective factors?

In answer to this question it may be noted that the actual shape of a curve depends upon the particular group of odors selected. With 16 perfumes of high affective value the curve would doubtless be skewed towards 100%; with 16 odors of low affective value the curve would be skewed towards 0%.[7] The present selection or odors was carefully made so that there would be an equal number of P and U odors and approximately equal gradations in general affective value (see Fig. 1). The group of odors selected does give the normal curve of distribution but even closer approximations could be made by changing the series—omitting or adding odors, or changing their order of presentation. In other words, there is nothing sacred about the normal curve. We have deliberately tried to find a series of odors which would give a normal distribution of likes and dislikes and, as it happens, we have been moderately successful.

---

[4] J. G. Beebe-Center, General affective value, *Psychol. Rev.*, 36, 1929, 472.

[5] There is, of course, a difference between an *S* who finds all odors I, and the one who is pleased by half and displeased by the other half of them. Individuals differ in the degree to which they are pleased-or-displeased by odors, but differences in affective sensitiveness are not here considered. Cf. M. F. Washburn, H. Clark, and N. Quackenbush, A suggested coefficient of affective sensitiveness, this JOURNAL, 24, 1913, 583-585.

[6] Weighting was based upon the general affective values of the odors. It is obvious that an *S* pleased by No. 13 (only 7% P) is more easily pleased than an *S* reporting No. 3 (93% P) as pleasing.

The scale of intensive values also offers a possible means of weighting the scores. This method was not used.

[7] If instead of odors we had used the common foods, the curve would be skewed towards P. For evidence see P. T. Young, *op. cit.*, 24-5.

One further point needs to be noted. An *S*'s tendency towards being pleased or being displeased has been indicated by a single statistical value. This method of treating the results ignores the fact that the experience of being pleased is different from the experience of being displeased, and the corresponding physiological processes are also different.

Summary. A series of chemically pure organic substances was selected to give uniform gradations between very pleasant and very unpleasant. These odors were presented to 100 *S*s under standardized conditions. The individuals were found to distribute normally as regards the tendency to be pleased by the odors.[8]

B. Position in Series as a Condition of General Affective Value

Problem. It was shown by Beebe-Center that the affective value of an odor depends upon its position in a series.[9] Whether this fact be interpreted in the older terms of contrast or in the newer terms of configuration need not detain us here.[10] From the practical point of view it means that the total group of odors, rather than the single odor, must be regarded as a unit in a test of affective reaction.

Method. To study how far position in the series determines the percent P of an odor a special experiment was carried through. Two series of odors were arranged as follows:

Series A		Series B	
Odor No.	% P	Odor No.	% P
9	81	12	15
2	90	11	9
3	93	13	7
13	7	3	93
11	9	2	90
12	15	9	81
16	61	16	61

The members of the series and "% P" were taken from Part A. Series A consists of three very P odors in order of increasing %P, followed by three very U odors in order of increasing %P (decreasing unpleasantness). Series B contains the same 6 odors in reverse order. The critical point in both series is the transition between Nos. 3 and 13—the most P and the most U odors. How does the percent of these critical odors vary with position in the series?

[8]For purposes of testing an abbreviated list of odors might be desirable and would probably still give the normal curve. We suggest the following 8 odors in the order given: Nos. 7, 14, 6, 4, 13, 1, 3, 15.

[9]*Op. cit.*, 54 f.

[10]A. J. Harris, An experiment on affective contrast, this Journal, 41, 1929, 617 f.

Both series are concluded by the empty bottle, No. 16. In Part A this was most frequently reported as "no odor at all," "indifferent." One might expect the affective reaction to an empty bottle to be indifferent,[11] but in the context of odors it was sometimes P and sometimes U and the percent P in Part A was 61. How does the percent P for the control, No. 16, vary with its position in the series?

A group of 100 *Ss*, exclusive of the group in Part A but comparable in every respect, was used. The conditions of presentation were the same as those of

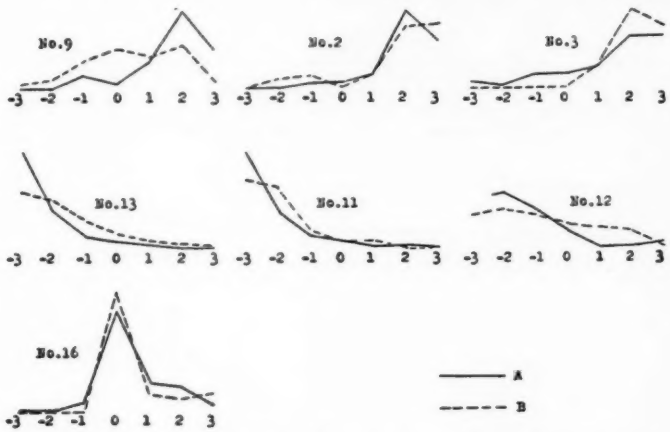


FIG. 3. DISTRIBUTION OF THE THREE MOST AND OF THE THREE LEAST PLEASANT ODORS, AND OF THE CONTROL, OVER THE SCALE OF AFFECTIVE VALUE

Part A except for the series of odors. Series A was given to the 50 even-numbered *Ss* and B was given to the 50 odd-numbered *Ss*. The two series were thus used alternately throughout the experiment.

Results. The results are presented in Table I and graphically in Fig. 3. A scale of values based upon the *Ss*' reports has been used as follows: -3 = very U; -2 = moderately U; -1 = weakly U; O = indifferent; 1 = weakly P; 2 = moderately P; 3 = very P. This scale is shown in Table I and as a base line of Fig. 3. The number of reports of each intensive degree gives the ordinates in Fig. 3.

Inspection of the data brings to light the fact that all three U odors, Nos. 13, 11, 12, are more frequently 'very U' in Series A (following P odors) than in Series B (standing alone). An increase of

[11]The empty bottle was "the most indifferent of the series" in our previous study but there were only 18 *Ss*, *op. cit.*, 19.

intense U necessarily means a relative decrease in other parts of the scale since the total number of *Ss* is limited. Odor No. 12 has greater frequencies for all degrees of U in Series A than in Series B and corresponding reductions on the P side of the scale. We may generalize as follows: after a series of three pleasant odors all three unpleasant odors were more intensely unpleasant than when standing alone. This has been called a "contrast effect."

TABLE I

DISTRIBUTION OF AFFECTIVE JUDGMENTS ALONG A SCALE OF VALUES AS DEPENDENT UPON ORDER OF PRESENTATION

(Fifty *Ss* judged in each series. Differences between Series A and B depend presumably upon the positions of the odors in the series)

		Scale of values						
Odor No.	Series	−3	−2	−1	0	1	2	3
9	A			4	2	8	24	12
	B	1	3	8	12	10	13	3
2	A	1	1	2	2	5	24	15
	B		2	3	1	5	19	20
3	A	2	1	4	4	7	16	16
	B				1	7	23	19
13	A	31	12	4	2	1		
	B	18	15	9	5	2	1	
11	A	30	12	4	2	1	1	
	B	21	19	6	2	2		
12	A	15	17	12	5			1
	B	10	12	10	7	6	5	
16	A			2	30	9	7	2
	B				36	6	3	5

With P odors (Nos. 9, 2, 3) the result is not so uniform. The critical odor, No. 3, is more P in Series B (following U odors) than in Series A (third in the group of P odors). This confirms our expectation on the basis of a 'contrast effect.' The contrast effect, however, does not last very long for No. 2 is about the same in both series. The effect is present with No. 2 but it is small and of doubtful significance. When we consider No. 9 a marked difference is apparent between the two series but the direction of the difference is opposed to our theoretical expectation on the basis of a contrast effect. No. 9 is distinctly more P when it stands alone at the start of Series A than when it comes towards the close of Series B. Is this 'counter-contrast?' Further work is needed to clear up the matter.

The empty bottle gives about the same result in both series. Curiously enough there are more reports of P than of U for the control. In the Series A the control is 66% P and in the Series B it is 64%. It is still an open question as to how far the affective reaction to an empty bottle depends upon position in the series.

Discussion. To see whether the differences between Series A and B are genuine and significant we have gone through the data of Part

TABLE II

THE VARIATION OF TWO GROUPS AS DEPENDENT SOLELY UPON SAMPLING

(The 100 *Ss* were divided into two groups on the basis of odd and even numbers. The differences between the groups are random differences of sampling).

Odor No.		Scale of values						
		−3	−2	−1	0	1	2	3
9	odd		1	5	5	8	23	8
	even		8	2	4	10	18	8
2	odd	3	1		4	1	27	14
	even		1	3	3	6	17	20
3	odd		1		4	7	15	23
	even			1	6	7	16	20
13	odd	32	10	4	2	1	1	
	even	32	12	1	3	1		1
11	odd	34	7	2	2	2	2	1
	even	38	6	3	1	2		
12	odd	19	16	6	4	3	2	
	even	17	16	7	5	3	2	
16	odd			1	34	9	4	2
	even				42	2	3	3

A making curves for the same seven stimulus-objects. One curve was made for the 50 odd-numbered and another for the 50 even-numbered *Ss*. Data for plotting these curves are given in Table II.

Odds and evens differ, but the differences are not consistent in direction as they are in Table I and Fig. 3. Further, the magnitude of the differences is generally less than in the case of the curves of Series A and B. We conclude, therefore, that the differences between the results of Series A and B are not dependent upon chance variation alone.

One positive point should be stressed. The similarity of Tables I and II is much more striking than the differences in question. This means that an odor has a general affective value which to a large extent is relatively independent of its position in a series.

Summary. A control experiment has shown that the affective reports for a group of *S*s vary with the position of the odor in a series. A U odor is more intensely U when it follows P odors than when it stands alone; a P odor is more intensely P when it immediately follows a group of U odors than when it stands alone. Several matters need further investigation, specifically: (1) the difference between P and U in permanence of the 'contrast effect' and possible "counter-contrast," and (2) the fact that an empty bottle in the context of odors gives an affectively pleasant reaction.

An odor has a general affective value which to a large extent is independent of its position in the series.

General Conclusions

The total experiment reveals some of the basic requirements for a test of individual differences in the tendency to be pleased and displeased. Constant stimuli need to be used under standardized conditions of presentation. The series of stimuli should be so selected that the *S*s will give a normal curve of distribution. We have found that a linear distribution of general affective values from very U to very P gives a normal type of curve. The order of presentation must be kept constant for a given test. The present work indicates that the general affective value of an odor varies with its position in a series.

XII. The Relation between Age and Affective Reaction to Odors

The aim of the present study is to determine whether likes and dislikes for chemically pure substances vary with age.

Subjects. Children in the schools of Champaign, Illinois, served as *S*s. One hundred and forty-four children were tested, but the present report is based on the record of 50 children within the age group 7 to 9 years inclusive, and 50 within the age group 11 to 13 years inclusive. Comparable adult groups are taken from the work described in the foregoing paper.

Method. Fourteen chemically pure substances[12] were used as stimuli with an interval of 30-60 sec. between successive presentations.[13] The work was done in

[12]The substances, with two omissions, were the same as in the foregoing experiment. The order and sequence for children and adults was the same. Since the work with children was carried on at the same time as that with adults a second independent series of bottles was made up. The number of grams of each substance was: Odor No. 1 (23), No. 2 (27), 3 (15), 4 (12), 5 (13), 6 (20), 7 (18), 9 (29), 10 (24), 11 (17), 12 (24), 13 (17), 14 (28). Nos. 8 and 15 of the original list are omitted, but No. 16, the empty bottle, was in both lists. Work with these odors showed that No. 5, menthol, had a slight cutaneous irritation to the children.

[13]The interval had to be as short as possible to keep the children from becoming restless and bored. On the other hand, we desired to avoid adaptation and mixture effects.

the rest room which was specially arranged for the experiment. The window was raised three inches from the bottom and lowered three inches from the top, and the bottles were placed on a table in front of the sill. This arrangement created a current of air which passed over the bottles and rose to the top of the room, passing out the upper opening. Every precaution was taken to keep the bottles clean, to keep the substances invisible to the child, and to maintain constant experimental conditions from day to day so far as possible.

FIG. 4. GENERAL AFFECTIVE VALUES OF ODORS FOR DIFFERENT AGE GROUPS

The child was seated with his back towards the bottles, and instructed as follows:

I have several bottles here with a different odor in each one. I want you to take a big smell, like this [inspiration demonstrated by experimenter], and tell me whether you like it or whether you don't like it.

After the instruction an odor was presented to the child. His verbal response, bodily expression, and time of response were recorded. A record was also kept of the child's name, age (to the nearest birthday), school grade, and of other conditions such as weather, attitude and mood of the child.[14]

Results. (*1*) *Affective value.* Table I shows the percent for each odor in the series and for the three age groups. The figures for adults are based upon 100 cases which are strictly comparable as regards stimuli used, order of presentation, and other conditions.

[14]During the winter months some of the children had colds. If the cold was bad, the child was returned to the class room, but if slight, the fact was recorded on the mimeographed sheet and the experiment carried out.

Fig. 4 shows the 14 stimulus-objects arranged in a rank order from the highest to the lowest percent. The order for adults is used as a base. Inspection of Fig. 4 indicates a high degree of consistency among the three age groups. Apparently the general affective value of an odor is relatively independent of the age of the group of *Ss*.

Correlations between these three rank orders are extremely high. By the Pearson standard product-moments formula (computed by Miss Morgan) the coefficients are: Groups I and II, r = 0.93; Groups I and III, r = 0.91; Groups II and III, r = 0.96. By the rank-difference method of Spearman (computed by the senior author) the

TABLE III

PERCENTAGE OF 'PLEASANT' JUDGMENTS FOR THREE AGE GROUPS
(Groups I and II contain 50 *Ss* each; Group III, 100 *Ss*).

Serial No.	Odor No.	Group I (7-9)	Group II (10-13)	Group III (18-24)
1	9	80	79	81
2	12	10	12	15
3	7	54	48	56
4	14	30	24	20
5	10	47	32	31
6	6	47	52	31
7	16	82	67	61
8	11	26	14	9
9	4	76	86	83
10	2	84	94	90
11	5	56	52	62
12	13	38	8	7
13	1	66	78	70
14	3	84	89	93

coefficients are: Groups I and II, r = 0.93; Groups I and III, r = 0.92; Groups II and III, r = 0.98. These correlations clearly indicate that under present conditions the age of the subject does not matter so far as his likes and dislikes for chemically pure substances are concerned. This high degree of constancy may be attributed to physiological mechanisms which are relatively mature at the age of seven years.

(2) *Age differences.* In some cases the youngest group seem to have slightly higher percentages of P than the older groups, but there are many exceptions. Possibly a rough index of this tendency is the arithmetical sum of all percentages. The totals are: for Group I, 778; for Group II, 735; for Group III, 709. If further work should show that the difference is genuine and significant the question of interpretation would remain. Are young children more easily pleased

then adults; or do suggestion and other non-affective factors account for the result? In the present case these questions are open.

In the case of the empty bottle, No. 16, the youngest group is more readily pleased than the adult group. The percentages are: Group I, 82%; Group II, 67%; Group III, 61%. Theoretically one would expect the empty bottle in a context of odors to be indifferent, *i.e.*, 50% P, but in the present and previous study it has been consistently P.[15] With children this tendency is even more marked than with adults.

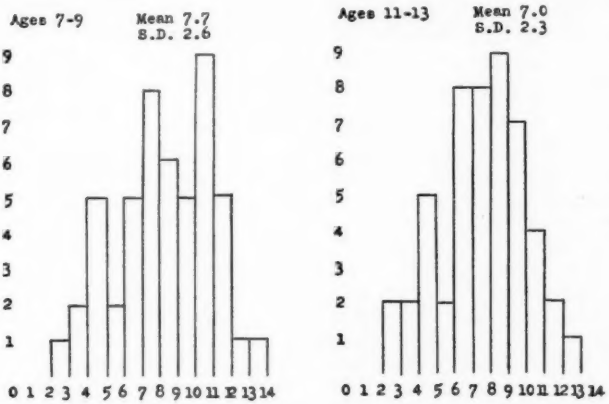


FIG. 5. DISTRIBUTION OF RAW SCORES FOR GROUPS OF CHILDREN

(3) *Individual differences in affective reaction.* A raw score for every individual has been determined which indicates the number of stimulus-objects reported as pleasant. The maximum is 14 and the minimum is 0. Indifference and the failure to report are counted as 0.5. These raw scores have been plotted and are presented graphically in Fig. 5.

The shape of these curves approximates that of the normal curve of probability, and hence our work with children confirms the fore-

[15]We are inclined to account for this fact on the ground of its position in the series of odors. The empty bottle is immediately preceded by three distinctly U odors. Under these conditions the so-called contrast effect should render the empty bottle slightly P, and that is exactly what happened. The *Ss* were not told that an empty bottle was in the series; they were predisposed to smell something and report P or U. Under these conditions the control was P. Further experiments are needed to clear the matter, especially in view of the fact that the empty bottle was P in the Series B of the foregoing experiment.

going study with adults. The curve for the younger group is slightly more irregular than that for the older group and is skewed towards the P end of the scale. Once again there is a hint that the younger groups are slightly more ready to report 'pleasant' than the older group.

The curves for weighted scores (not given here) indicate that the younger children are more variable in their reports than the older children. It seems likely that this variability of small children depends upon their suggestibility, lack of attention, and other non-affective factors. Possibly also small children are more variable in their physiological affective reactions, but one cannot draw this conclusion from the data at hand. The present work indicates that children and adults are very similar in their affective reactions.

Corollary Results

(*1*) *Positivism and negativism.* Of the 144 children tested in the Lincoln school 9 reported that they liked all odors (100% positive) and 2 that they disliked all of them (100% negative). Six of the positive cases and both negatives were given special study.

The test was repeated without change of conditions for seven successive days. Five of the 6 positives remained 100% positive for the seven days. The other "liked everything" for the first three days, but on the last four days there were 1, 1, 4, 6 reports of dislike. The negative cases in both changed on repetition. One of them, a girl 6 years old, reported 100% dislike for three days, but for the remaining four days her positive score increased daily to seven. The other case of 100% dislike, a girl of 7, reported dislike of all odors for two days. On the third day 10 of the 14 reactions were positive, and on the remaining four days all of her reports were positive! This complete reversal from 100% negative to 100% positive makes one hesitate to interpret these cases in affective terms.

The study of positivism and negativism was extended to another school. No cases were found in the second grade, but in the first grade 6 of 30 children were 100% positive. The phenomenon in question is clearly a characteristic of the younger children.

The test was repeated for these positives under changed instruction. On the first repetition the child was asked after each report whether he would like to smell it again. If he said "no," we assumed that his original report of liking was probably ambiguous. On the second repetition definite suggestion was tried. The child was instructed as follows: "We are going to play a game to-day. Some of these smells are good and some of them are bad. See if you can pick out the bad smells and tell me when you find one." Two of the six '100-percenters' changed when the first variation of procedure was tried, two others changed with the second variation, and two remained consistently positive throughout the three trials.

Our experience indicates that failure to understand instructions, desire to please the experimenter, suggestion, and other non-affective factors are partly

responsible for cases of positivism and negativism. Several of the positive cases, however, appear to be genuine and straightforward. If we take these cases at face value, they support the conclusion that children are more easily pleased than adults.

It has been difficult to know whether cases of positivism and negativism should be included or excluded in treating the data statistically. As a compromise we we have made every calculation twice—once with the '100-percenters' included and once with them excluded. Practically it doesn't much matter which course we take so far as results are concerned.[16]

(*2*) *Expressive movements.* A record was kept of every child's bodily expressions of pleasure and displeasure while smelling the odors. The behavior which goes with liking an odor includes: smiling, speaking in a 'pleasant' tone of voice, laughing, nodding head, opening of the mouth, looking up, slight sucking movements (one case), taking a longer and deeper smell of the substance. Expressions of dislike are more numerous and include: turning away the head in various degrees, turning away the entire body, jerking head back suddenly, wrinkling nose together with raising the upper lip, shaking the head, speaking with disgust ("ugh," "uff," "ooo," "ahh"), leaning back in chair away from bottle, twisting mouth, speaking very definitely 'no,' clearing throat and coughing, compressing lips, rubbing nose with finger or arm, frowning, putting hand over mouth or nose, pinching nose, spitting, wrinkling face, a 'pained, unhappy look,' waving the bottle away with the hands.[17]

(*3*) *Affective response-times.* Without the knowledge of the *Ss* the time between presentation of the odor and verbal response was measured with a stopwatch. These times are in no sense reaction-times. Careful study of the record shows that the first is generally the longest and there is a shortening or 'settling down' effect. This habituation effect is so pronounced that it obscures differences dependent upon P and U.[18]

---

[16]In Table 4 and in Figs. 4 and 5, these questionable cases have been excluded. When the doubtful cases are included there is a correlation between all children and all adults of 0.95 (Pearson's formula). Inclusion changes the shape of the curve for ages 7-9 in Fig. 4 by extending the 14-15 column up to six. The high column suggests that the cases do not belong with the normally-distributed group but are in a class by themselves.

[17]In a recent experiment by Miss Marguerite E. Gauger (The modifiability of response to taste stimuli in the pre-school child, *Columbia Univ. Contrib. to Educ., Teachers Col. Series*, 1929) the criteria for liking tastes were: "Chewing, smacking lips, licking lips, smiling, eyes lighting up, reaching for stimulus, swinging feet, moving head, sticking out tongue, speaking ("that's good"), retaining stimulus as long as possible to prolong pleasure. For dissatisfaction: Making a wry face, frowning, trying to spit out the stimulus, tears in eyes, speaking ("I don't like that"), coughing, gagging, vomiting, turning away head, pushing away stimulus, refusing to give attention, retaining stimulus as short a length of time as possible, looking hurt as if he wanted to blame the experimenter for the bad taste." Due to the similarity between taste and smell many of these are similar to those found for smell stimuli.

[18]Affective response-times were also taken in the foregoing experiments with adults. The first odor in the series had a response-time of 9.2 sec. with an A.D. of 4.2 sec. The first seven odors had an average time of 7.0 sec. and the last nine an average of 5.4 sec. These differences, which depend upon habituation, are so

(*4*) *Constancy of affective reaction.* During the Christmas vacation the experiment was repeated for 8 consecutive times with 4 *Ss*. The total record shows a very slight increase in the number of positive scores with the progress of the experiment. At the conclusion the children were asked if the odors seemed more or less pleasant than at the beginning or just the same. All four replied, "more pleasant," but the small number of cases makes the result unreliable.[19]

In the experiments with the adults the tests were repeated 6 times for a group of 13 *Ss* selected from the original 100. The intervals between successive repetitions were equal (two days) whenever possible. The results show that affective reactions to odors have a good degree of constancy from day to day. Since this confirms previous work details need not be given.[20]

(*5*) *Associations.* Both children and adults reported a good many associations despite the fact that they were not specifically instructed to do so. Study of the record shows an appreciable similarity of the age groups. For example, vanillin to the children suggested candy, chocolate, vanilla wafers, vanilla ice cream, cream, marshmallows, and powdered sugar; to the adults it suggested vanilla, chocolate, and candy. Methyl salicylate to the children suggested peppermint, wintergreen, gum, candy, medicine, perfume, and paste; to the adults it suggested wintergreen, peppermint, mints, and liniment. Phenol to the children suggested medicine, dentists, doctors, lysol, carbolic acid, paste, and alcohol; to the adults it suggested medicine, operating room, vaseline, antiseptic, carbolic acid, and paste.

It is an interesting question as to how far previous affective reactions determine present ones. Subjects sometimes make such remarks as " I like it because it reminds me of geraniums." "I dislike it; it's just like the medicine I had to take." These remarks indicate the need of further study on the problem.

---

marked that they obscure other possible results and there is zero correlation between percent P and response-time. If we limit our consideration to the last nine odors, assuming that the habituation effect is greatest at the start, a possible result appears. The two most U odors have also the shortest response-times—No. 11, 3.9 sec., and No. 13, 4.1 sec. The two most P odors are next to the shortest in response-time—No. 3, 5.0 sec., and No. 2, 5.0 sec. Odors near indifference have somewhat longer response-times. This would seem to indicate that the more intense the affective reaction the shorter the response-time. In a thorough study of the problem habituation would have to be controlled and some reliable measure of reaction-time used.

[19]It is possible that the children may have gained the impression that repetition was to make them like the odors; one child, at least, said: "You never will make me like that second odor." Both Miss Kniep and Miss Morgan reported that the odors seemed less disagreeable at the close of the experiment than at the start. Inasmuch as no physical and chemical changes in the substances were detected in the laboratory, we may assume that the gradual change in affective value indicates adaptation.

[20]P. T. Young, Constancy of affective judgments to odors, *J. Exper. Psychol.*, 6, 1923, 182; also this JOURNAL, 42, 1930, 17 ff.; J. H. Kenneth, A few odor preferences and their constancy, *J. Exper. Psychol.* 11, 1928, 56-61.

General Conclusions

Chemically pure odorous substances were presented to school children under controlled conditions and their affective reactions studied. The experiment shows that children and adults are similar in their likes and dislikes for odors; all correlations between age groups are above 0.90. Raw scores on the test give a good approximation to the normal curve as in the case of adults.

There is a hint that children are more readily pleased than adults, but the result may depend upon suggestion and other non-affective factors.

Among incidental results the most important is the discovery that some children of ages 6,7, and occasionally 8 and 9 years (younger ages not studied) report that they like all of the odors or more rarely that they dislike all of them. The evidence in some of these cases indicates that non-affective factors partly determine the result.

GROUP PRESENTATION IN THE METHOD OF CONSTANT STIMULI AS A TIME-SAVING DEVICE

By Dorothy J. Shaad and Harry Helson, Bryn Mawr College

The adjustment of data to the best-fitting ogive curve in the method of constant stimuli by the use of Urban's tables and formulae is of necessity a time-consuming task, even though one is spared by these means the trouble of setting up and solving the normal equations entailed by the method of least squares in curve-fitting. Further reductions in the time necessary to obtain a limen have been made by reducing the number of stimuli and the number of series;[1] but reducing the number of stimuli or the number of series or both means that the total number of observations have been reduced and the probable error of the limen increased. Since, as Boring has pointed out,[2] it is the reliability of the obtained limen that matters and this is a function of the number of observations, no further reduction in this part of the work can at present be made. It would therefore appear as if the only way left to reduce the time spent in obtaining limens by the method of constant stimuli must come, as Fernberger has said,[3] "in the line of reducing the amount of work required for making the observations. This may be done either by an improvement in the technique of experimentation or by reducing the number of . . . . stimuli used." Nevertheless, a limen which is not statistically reliable is useless, even though obtained by the most meticulous mathematical treatment. We may therefore regard five stimuli and 100 series (500 observations in all) as the lowest number that can

*Accepted for publication August 30, 1930. This study was prompted by difficulties experienced in the investigation of the two-point limen in Studies of Nerve Regeneration by the senior author and K. M. Dallenbach soon to appear.

[1]S. W. Fernberger, The simplification of the practice of the method of constant stimuli, this Journal, 25, 1914, 121-130; On the elimination of the two extreme intensities of the comparison stimuli in the method of constant stimuli, *Psych. Rev.*, 21, 1914, 335-355; The effects of practice in its initial stages in lifted weight experiments and its bearing on anthropometric experiments, this Journal, 27, 1916, 261-272.

[2]E. G. Boring, The number of observations upon which a limen may be based, this Journal, 27, 1916, 315-319.

[3]First reference in footnote 1.

generally be safely used.[4] Time can be saved only in the technique of presenting the stimuli and making the observations. In this study we wish to determine if the time spent in making the observations can not be reduced without cutting down the total number of observations upon which the limen is based.

That any saving in time spent in making the observations is of great importance will be apparent to anyone who has tried to obtain data from children who quickly grow restive, from patients in a clinic who are apt to be bored or fatigued by the procedure, or with apparatus requiring constant re-adjustment in order to change the stimulus each time an observation is made. The time factor becomes even more important when all of the observations cannot, but should, be made at one sitting, *e.g.* in getting sensitivity to brightness, since it is almost impossible to begin again at exactly the same level of dark adaptation in another sitting.

The chance order of presentation of stimuli has been favored by most experimenters, quite rightly, because the curve to which the data are fitted is the integral of the normal probability curve. On theoretical considerations alone chance order within each series gives the best chance arrangement of stimuli, just as a deck of cards is said to be in a more random order after being shuffled than when it left the dealer arranged in a progression of suits. It should be remembered, however, that any arrangement is one out of all possible arrangements by chance, regular order being no more unlikely than any other. If we are fitting data to a probability function it is natural to attempt as random, or unexpected, an order of stimuli as possible. If we repeat a given stimulus a number of times in succession, forming a group, but choose the groups haphazardly we shall also have a chance arrangement, albeit not as unexpected an order as by the usual method. In the method of groups we have, as Titchener has pointed out,[5] one of the possible ways of presenting stimuli in chance order for the method of constant stimuli.

By the group method, therefore, we do not mean the so-called method of serial groups which Thomson classifies with the method

[4]This seems to be Boring's conclusion for ordinary work when he says: "One does not ordinarily wish to take less than 100 series" (with five stimuli). Cf. Urban's tables and the method of constant stimuli, this JOURNAL, 28, 1917, 280-293, esp. 290.

[5]E. B. Titchener, *Experimental Psychology*, II, ii, 1905, 274. This author points out that it depends upon what we take as the unit of the experiment, the series, the experiment as a whole, or what not!

of minimal changes.[6] As Titchener shows, this is distinctly a form of the method of constant stimuli.[7] The data are therefore amenable to probability treatment even though chance order does not extend within each series. Fechner seems to have been the only one to use the group method to any extent.[8] The group method therefore has the authority of Fechner behind it even though it does not conform *throughout* to the presuppositions behind the *phi-gamma* function.

Our purpose in this study is to discover if there is any loss in precision or reliability in limens based upon the group method of presentation of stimuli as compared with the haphazard arrangement within each series. If there is no significant difference between the generally accepted method of presentation and the shorter, time-saving method, then the latter is to be preferred. Our investigations concern lifted weight comparisons and the two-point limen.

A number of questions immediately present themselves with respect to the group method. Should each stimulus be repeated the same number of times? What effect does knowledge that the stimuli are being repeated have upon the observer? What instructions should be given if the stimuli are repeated? These questions, and perhaps others, were answered by determining the limens for lifted weights and two points under four different methods or sets of conditions as follows.

Method I. One hundred series using chance arrangement within each series, the usual procedure.

Method II. One hundred series using the group arrangement in which each stimulus was repeated a regular number of times: 25 times for 5 *O*s and 10 times for 2 *O*s. As many as 25 repetitions were used because it was felt that any difference between the chance method and the group method would be much more evident with a large number of repetitions than with a small. In this set of experiments the *O*s were not informed that the stimuli might be repeated.

Method III. Same as Method II, but the instructions contained the added information: "The same pair (or stimulus) may be presented more than once in succession." Since the previous instruction had stressed independent judgment of every stimulus or stimulus-pair, it was repeated here.

Method IV. Same as Method III except that the stimuli were presented an irregular number of times in succession. Here the *O*s knew that the stimuli might be repeated.

---

[6] G. H. Thomson, A comparison of psychophysical methods, *Brit. J. Psychol.*, 5, 1912, 203-241. In the method of serial groups, as he points out, the order of succession is consecutively ascending and descending. Thomson in this article introduces a new method, the method of non-consecutive groups where no regular progression of groups is made. It differs from our method of groups in that we regard our method as a special case of the method of constant stimuli, from the point of view both of the logic of the experiment and of the method of calculating the data.

[7] Titchener, *loc. cit.*

[8] See Titchener, *loc. cit.*

The use of an irregular number of repetitions in each group was to prevent *Os* from learning when stimuli were to be changed and to prevent any set attitudes from forming.

Because of the possibility that the order in which the limens were obtained might affect the results, some variations were used for the different *Os*. Methods I and II, in which the *Os* were not informed that a given stimulus might succeed itself, necessarily preceded Methods III and IV in which they were informed. Since three of the *Os* were given group practice, the others being given practice by the usual method of presentation, the group method for them preceded the purely chance (usual) method. For all *Os* Method III preceded Method IV. One exception to this: *Sh*, who had chance practice, was given Method II before Method I, and Method IV before Method III. But since the two *Os* (the writers), who knew what the experiments were designed to test, were unable to tell which of the four methods was being used at any time, it is doubtful if method of presentation played any *conscious* part in determining the results. Our data bear us out in this conclusion.

Experiments with Lifted Weights

Procedure. Each *O* was given 100 series as practice with five comparison stimuli, making 500 judgments in all before regular experimenting was begun. The weights were lifted between the thumb and forefinger, the height of the lift being somewhat controlled by the necessity of keeping in time with the metronome, set at 54 beats per min. Four beats were allowed for each complete lifting of a single weight. The weight was lifted on the first beat, returned on the second, and followed by a rest of two beats before the next weight was lifted. The rest period gave the *E* time to change weights when necessary and to make the record, while the *O* was given time to make his judgment and to rest. This rate allowed about 6 comparisons per min. Brief rests were given after every 5 series at first and later after every 10 series.

Since the results of only one time order (standard first) have been used, time errors of the first order are present in all the results.

Directions. The directions given before the practice series were as follows.

You will be given a series of weights to compare. In each case, judge the second weight as heavier, lighter or equal to the first. Judge objectively, that is, without special regard for kinaesthesis, arm movement, or similar criteria. Do not start the experiment with any preconceived notions as to the order in which the weights may be presented. Judge each pair independently.

The category *equal* covered both the equal and doubtful cases.

The *Os* taking part in the experiments and their training were as follows: E. Fehrer (*Fe*), senior in psychology, somewhat experienced in observing; O. Futch (*Fu*), graduate in education, inexperienced; H. Helson (*H*), professor of psychology, experienced; T. M. Miller (*M*), graduate student in psychology, somewhat experienced; D. Shaad (*Sh*) and M. Small (*Sm*), graduate students in psychology, somewhat experienced; and Dr. E. F. Wells (*W*), demonstrator in psychology, experienced. Only *Sh* and *H* were aware of the purpose of the experiments.

For presenting the weights the Stoelting carrier bracket, which eliminates the space errors by bringing the stimuli to the same position each time, was used.

The height of the bracket was such that the *Os* could easily handle the weights as they stood before it blind-folded. The weights themselves were wooden cylinders, 6.4 cm. high and with a diameter of 3.7 cm., hollowed on one side to permit adjustments in the weights by the addition of shot during the course of the experiments. The weights were 101,103,105 (which was also the standard), 107 and 109 grm. The slight deviations found in the weights were so small as to be negligible.

Results. The results of the four methods are given in Table I wherein will be found the limens (L), the measure of precision (h), and the probable errors of the distributions (P), the latter being calculated by the formula: P = 0.4769/h. L and h were calculated in the usual manner by the use of Urban's tables and refer to the upper L, the percentages for *heavier* only having been used. Averages for all *Os* are given in the bottom row of the table.[9]

In using only one time order only h and P have significance and, judged by these average values, the pure chance method is not only best theoretically but also in these experiments, since Method I gives the highest precision (h) and the lowest P. For three *Os* (*Fu, M, Sm*) the Method II (without information that the stimuli might be repeated) gave extremely high probable errors and tended to make this method show up as the poorest, whereas without the values of these *Os* this method would have averaged very close to the other three. That these high Ps (hence low hs) are not simply the result of changing without warning from Method I to Method II is shown by the fact that *Fu* was given Method II before Method I. Unless these high values are due to some artifact, it means that this method encourages persistence in a given attitude for one of the

[9]But it should be noted that, when we come to decide if there is a true difference between *methods*, we calculate the probable errors of the *average* limen and the probable error of the difference between average limens. The alternative would have been, as Boring did in comparing limens, to calculate the probable error of each individual limen in each method. Since the limen is the median of the ogive distribution, a different probable error will result if we calculate each limen in each method and compare limen for limen, or the average of these probable errors. In comparing the average limens, we simply use the formula for the probable error of the difference between two *averages* and the difference between the average limens to determine if there is a reliable difference between them. We preferred this method because it enables us to treat each method as a whole, whereas by the other methods some limens will show a significant difference between two methods and others will not, with the result that we should have to average the values giving the probable correctness of a true difference to see if there is a real difference taking all *Os* together. The latter is a much longer procedure and we do not know that we are warranted in averaging probabilities in that way. Hence in Tables III and V the differences (D) are between average limens and the probable errors of the differences are calculated from the individual limens of which the average limens in Tables I and IV are the averages.

categories of judgment, which the *O* feels to be wrong, so that independent judgments are not made for each stimulus-pair. *H* and *Sh*, familiar with the experimental problem, and *W* an experienced *O*, did not give extremely high Ps in this method. Nor did *Fe*, who, while she was not an experienced *O*, proved to be an excellent observer in a number of other problems in which she took part. It would appear as if the average inexperienced *O*, fearing to give a long succession of same judgments, is led into changing his judgment or attitude. This conclusion is borne out by the fact that when the *O*s were informed

TABLE I

RESULTS FOR FOUR METHODS OF PRESENTING LIFTED-WEIGHT STIMULI
(L = limen; h = precision factor; P = probable error of the limen)

Os	I. Chance			II. Group (not informed)		
	L	h	P	L	h	P
Fe	106.9041	.101315	4.7071	105.4756	.088215	5.4061
Fu	105.2974	.106365	4.4836	102.8096	.037730	12.6398
H	102.4096	.058035	8.2174	102.8424	.086075	5.5405
M	105.7089	.096085	4.9633	104.7406	.021275	22.4159
Sh	104.1093	.114640	4.1599	106.0806	.060395	7.8963
Sm	104.5785	.104495	4.5638	100.8580	.027215	17.5230
W	105.6319	.089105	5.3521	104.0942	.078525	6.0732
Ave.	104.9485	.09572	5.2067	103.7811	.057060	11.0706

Os	III. Group (informed)			IV. Group (informed)		
	L	h	P	L	h	P
Fe	107.6262	.078770	6.0543	107.3912	.097290	4.9023
Fu	106.3196	.060420	7.8930	105.9660	.081575	5.8461
H	102.4576	.067045	7.1131	104.9235	.046535	10.2482
M	103.4997	.060590	7.8709	104.1184	.085650	5.5680
Sh	103.9494	.098210	4.8559	103.8934	.110240	4.3260
Sm	104.3524	.117040	4.0746	105.2328	.122945	3.8789
W	104.6485	.063860	7.4678	104.9320	.066240	7.1995
Ave.	104.6934	.063697	6.4762	105.7081	.087709	5.9955

that the stimuli might be repeated (as in Methods III and IV), no unusually high Ps (with only one exception, *H*, in Method IV) are to be found. In these cases the *O*s did not feel that they must be wrong in giving the same judgment a large number of times. Yet most of the *O*s reported when being given Method II that they felt that the same stimulus-pair was being repeated.

Table II shows the averages for *H*, *M*, and *Sm*, who were given Method I before Method II, and also the averages for *Fe*, *Fu*, and *W*, who were given Method II before the Method I. Whether Method II preceded or followed Method I, it gave a higher average P than Method I. It is not due, therefore, to the fact that the *O*s, having been trained on the chance method, become practised in it to the detriment of the group method.

From a comparison of the average Ps and hs in Table I, the four methods might be ranked according to their relative merits as follows: chance method, group method (informed) with irregular number of repetitions within each group, group method (informed) with regular number of repetitions, and, lastly, group method (uninformed) with regular number of repetitions.

We have still to determine if the superiority of the chance method is sufficient to overbalance the advantages of the group method in

TABLE II

COMPARISON OF FOUR METHODS OF PRESENTATION IN DIFFERENT ORDERS FOR LIFTED-WEIGHTS

Methods (in order)	Averages for *H*, *M*, *Sm*		
	L	h	P
I	104.2323	.086075	5.9148
II	102.8136	.044855	15.1598
III	102.4365	.081558	6.3529
IV	104.7582	.085043	6.5650
Methods (in order)	**Averages for *Fe*, *Fu*, *W***		
	L	h	P
II	104.1264	.068156	8.0397
I	105.9444	.098928	4.8476
III	106.1981	.067350	7.1385
IV	106.0964	.081360	5.9826

TABLE III

RELIABILITY OF DIFFERENCES BETWEEN FOUR METHODS OF PRESENTING LIFTED-WEIGHT STIMULI

Methods	D	P.E.diff.	D/P.E.diff.
I and II	1.1674	12.22	.09553
I and III	0.2551	8.30	.03071
I and IV	0.7595	7.93	.09578
II and III	0.9123	12.82	.07116
II and IV	1.9270	12.58	.01531
III and IV	1.0147	8.81	.01151

saving time, reducing the fatigue of changing or adjusting stimuli and simplifying the collection of data. If there is no significant difference between the chance method and the group methods, then it obviously pays to use one of the latter in many types of experiment. We therefore present in Table III the differences between every two methods of presenting the stimuli, the probable error of the differences, and the differences divided by probable error of the differences.[10] *The results of all possible combinations show no reliable chance of a true difference between any two of the methods.* In spite of the apparent superiority of the chance method, it is not sufficiently better to guarantee a

[10]See note 9.

difference in obtaining further limens. We believe, on the basis of these results, that the group method which is most convenient and time-saving may safely be adopted in pyschophysical procedure. But whatever method is used, *Os* should be trained and directions so formulated that independent judgments will be made on each stimulus pair. Our results show that this can be done in the group methods.

Experiments on the Two-Point Limen

After having come to the conclusions stated above with lifted weights we decided to work on the two-point limen where the group method is certainly more economical than the chance method.

Procedure. Stoelting aesthesiometers modified by adding sliding handles to keep the pressure constant were used in these experiments. An adjustable arm rest taken from the Mosso ergograph (in many ways superior to plaster of Paris casts) served to keep the arm in position. A screen of heavy black cardboard served to prevent the *O* from seeing his arm if his eyes were open, but most of the *Os* kept their eyes shut during the experiments.

Five of the 7 *Os* in the lifted-weight experiments continued in this part of the work. They were: *Fe*, *H*, *Sh*, *Sm* and *W*. An area 60 cm. by 20 cm. on the middle third of the volar surface of the left arm near the wrist was used for experimentation. This area was marked in India ink and re-marked at the beginning of each hour of observation.

Some preliminary work was necessary to determine what separations of stimuli would be satisfactory. For 3 *Os* (*H*, *Sh*, *W*) separations of 10, 20, 30, 40 and 50 mm. were found suitable; *Fe* required separations of 10, 15, 20, 25 and 30 mm., while *Sm* was given separations of 15, 20, 25, 30, and 35 mm. Although different stimuli were used for some of the *Os*, treatment of the data by the Urban method (or any method which yields h) makes their limens comparable.

After having determined the stimuli for each *O*, a practice period of 50 series (250 judgments in all) was given. It was found advisable to use *Vexierfälle* by the introduction of a single point occasionally. This helped to acquaint the *Os* with what a "one was really like" and aided in reporting *ones* when the fact was known that one point might be applied. In addition, before each period, *Os* were given one point and wide separations of two points to get a clear experience of 'oneness' and 'twoness.' While we do not wish to enter into the question of the merits of *Vexierfälle*, this procedure proved to be the only one possible to secure anything like constancy of judgment with our stimuli and our *Os*. Doubtful judgments were allowed and when given were noted, but the stimulus was repeated later. Circles containing two points were called *one*, but ellipses with points at the ends were judged as *two*.

Directions. The following directions were read to the *Os*.

You will be given a series of cutaneous stimulations, each of which is to be judged as *two* or *one*, depending upon whether you feel two or only one point of stimulation. In case you cannot distinguish the number of points, a judgment of *doubtful* may be given. An immediate and independent judgment is expected for each stimulation.

All *Os* were given chance order practice, and the methods were as follows:

Method I. One hundred series in which the separations were varied by chance within each series.

Method II. One hundred series by the group method in which each stimulus, selected by chance, was repeated 10 times, but in which the *Os* were not informed of this procedure.

Method III. One hundred series, same as method II, but with the information that the stimuli might be repeated.

Method IV. One hundred series in which the *Os* were informed that the stimuli might be repeated, but in which the stimuli were repeated an irregular number of times, instead of 10 times as in Methods II and III.

While all *Os* were given chance practice, as stated above, there was some variation in the order in which the methods were used for different *Os*. *Fe*, *H*, and *Sm* were given the methods in the order I, II, III, IV. The order for *W* was: II, I, III, and IV, while for *Sh* it was II, I, IV, and III.

Results. The results are summarized in Table IV which contains the limens (L) for each *O*, the measure of precision (h), and the probable errors of the distributions (P). The average L, h and P are given in the bottom row of the table.

TABLE IV

RESULTS FOR FOUR METHODS OF PRESENTING STIMULI FOR THE TWO-POINT LIMEN

Os	I. Chance			II. Group (not informed)		
	L	h	P	L	h	P
Fe	14.7788	.063104	7.5573	13.1250	.042190	11.3034
H	20.7157	.044921	10.6164	16.4050	.041314	11.5433
Sh	28.2204	.042283	11.2787	28.4551	.035030	13.6140
Sm	28.0878	.018669	25.5450	32.5505	.047954	9.9449
W	35.3225	.056042	8.5096	26.3836	.044798	10.6455
Ave.	25.4250	.042445	12.7013	27.3838	.045556	11.4102
		[.051587]	[9.4905]			

Os	III. Group (informed)			IV. Group (informed)		
	L	h	P	L	h	P
Fe	9.6995	.042408	11.2455	8.0655	.046104	10.3440
H	18.8030	.039883	11.9574	16.6140	.033747	14.1316
Sh	32.3335	.039310	12.1317	36.0821	.039561	12.0548
Sm	33.9880	.052878	9.0188	24.4772	.034436	13.8488
W	35.7445	.068741	6.9376	38.8610	.071578	6.6626
Ave.	26.1137	.048644	10.2582	24.8199	.045085	11.2083

Examination of the individual results reveals little consistency in the limens for the different *Os*. The group methods give lower limens than the chance method for *Fe* and *H*. From the results with lifted weights we should expect the chance method to give the smallest P and the largest h and this would indeed be the case were it not for the extraordinarily high value of P for one *O* (*Sm*). In Table IV, below the average h and P for all *Os*, is given the average h and P in brackets for the *Os* without the disturbing values for *Sm*, and it is

then seen that the chance method shows up best. Since the disturbing value of P is almost double any other single P in all four methods, we are probably justified in regarding it as an artifact of some sort. After giving the chance method its due, we still find that there is no significant difference between it and the other shorter methods. A comparison of L, h and P shows little gross difference among the four methods.

The fact that the L is lowest in Method IV may be due to practice since it was given last with all but one *O*. On the other hand, we do not find the large average P for Method II in this set of experiments that we did in the lifted-weight experiments. This leads us to believe that the poor position occupied by Method II among all the methods in the lifted-weight experiments may be due to chance, aside from

TABLE V

RELIABILITY OF DIFFERENCES BETWEEN FOUR METHODS OF PRESENTING STIMULI FOR THE TWO-POINT LIMEN

Methods	D	P.E.$_{diff.}$	D/P.E.$_{diff.}$
I and II	1.9588	17.07	.11469
I and III	.6886	16.37	.04201
I and IV	.6050	16.99	.03561
II and III	1.2701	15.33	.08285
II and IV	2.5639	16.17	.01585
III and IV	1.2938	15.18	.08523

the fact that we found no significant difference between it and the other methods. It is clearly evident that the results for the two-point limen are much more consistent than those we obtained in the first part of the work. We can account for this state of affairs by the following considerations.

(1) All *O*s had taken part previously in the lifted-weight experiments and had therefore had the group methods. If there is any tendency for successive same judgments to upset an *O*, by this time all our *O*s must have become quite habituated.

(2) The use of *Vexierfälle* may also have helped to stabilize attitude by forcing the *O* to make each judgment independently of the others. The insertion of *ones* helped also to break up a long string of similar judgments.

(3) In these experiments the number of regular repetitions was kept at 10 instead of 25 as in the lifted weights. Smaller groups may help to give more consistent results.

When we come to the statistical criterion for a true difference between our methods we find in Table V, where the differences be-

tween the average Ls, the probable error of the differences and the ratio of the differences to the probable error of the differences are given, that there is no reliable chance of a true difference between any of the methods.[11] The ratios vary from 0.015 to 0.114, whereas a reliable difference is supposed to require 3 or more. We must conclude that if one does not wish to change stimuli each time in getting a two-point limen, he need not do so and it does not matter whether he informs the *O* or not, provided that the *O* makes each judgment independent of those that have gone before. For this purpose *Vexierfälle* are recommended as helping to prevent attitudes from being formed and to make independent judgments easier. If there is any tendency for *O*s to learn the number of repetitions in a group, Method IV tends to break this up. With the use of zero separations occasionally and irregular groups, Method IV comes very close to being almost identical with the chance method so far as the *O*s' knowledge of procedure is concerned.

The results for the two-point limen are more favorable to the group methods than those for lifted weights, although in both sets of experiments no significant difference in favor of the chance method was found. It then becomes a purely practical question as to when we shall use the chance or group methods of presentation of stimuli. If, as in the lifted-weight experiments, the group methods do not result in saving time owing to the fact that the apparatus is usually so simple to arrange that the time element hardly enters at all, then we should naturally use that method which conforms most closely with the theoretical considerations upon which the function, which we are trying to fit, is based. Where the apparatus is such that a change of stimulus in every observation lengthens the time necessary for collecting the data, one of the group methods is justified for the following reasons. (1) Since our time-saving device does not consist in reducing the number of observations either by reducing the number of stimuli or the number of series, the reliability of the obtained limen, which is a function largely of the number of observations, has not been altered. (2) No significant differences have been found in the results by the chance and the group methods. (3) We are actually employing a chance arrangement of stimuli, the order of succession of groups, instead of the single stimuli within each series, being haphazard. The possibility of chance arrangement of *groups* has been

[11]See note 9.

recognized by all authorities in psychophysics from Fechner on, although little use of it has been made in the method of constant stimuli and its historical progenitor, the method of right and wrong cases.

Summary and Conclusions

The chance and group methods of presenting stimuli in the method of constant stimuli have been compared to determine whether or not the group method, which saves time in many types of experiment, gives results which justify its use. No significant differences were found between the chance method and any of the group methods, either with respect to h or P, both in lifted weights and two-point limen.

In those types of experiment where the group method saves time for making the requisite number of observations necessary for a statistically reliable limen, one of the group methods we have used is to be recommended. Method IV seems to be the best of the group methods because, with an irregular number of repetitions, it approaches most closely to the chance method of presenting stimuli, which we admit, on the whole, to be slightly preferable—mostly on theoretical considerations. *Vexierfälle* and other devices can be adopted to insure independent judgments. In view of the widespread use of the method of constant stimuli outside the psychological laboratory, time-saving procedures are a necessity. We have found the method of groups highly advantageous in this respect.[12]

[12]One of the writers (*H*) has been using the group method with untrained *O*s in a medical practitioner's office with results fairly comparable with the chance method used with subjects in the psychological laboratories. At least the differences found have not been attributable to the method of presenting the stimuli, and the saving in time has made the work endurable to laymen.

THE EFFECT OF MENTAL EFFORT ON PAROTID SECRETION

By A. L. Winsor, Cornell University

The conditioned reflex technique of using quantitative variations in secretion as an objective evidence of cortical behavior can have little significance with human subjects until the general relationship of mental activity to secretory function is taken into account. Although some studies of the influence of mental effort on motor reflexes have been reported, very little consideration has been given to a like influence upon glandular reflexes. The results of those that have been attempted appear to be contradictory.

Brunacci and De Sanctis,[1] using a dilute solution of acetic acid to stimulate their subject once a minute, recorded the resultant secretion when the subjects were at rest and when they were translating from a foreign language or computing. They noted that the quantity of saliva was smaller during mental activity than during repose, and interpreted their data as an evidence of the *inhibitory* action of mental activity. Lashley questioned the reliability of their technique and conducted an experiment using the normal secretion as a control.[2] He required his subjects to square three-place numbers for 20 min., to remain quiet for 20 min., and then to resume computation for 20 min. The average number of drops during the period of rest was found to be less than during computation. This experiment was interpreted as an evidence of the *excitatory* nature of mental activity.

In our work with parotid secretion we have had occasion frequently to notice the effect of mental effort on the rate of this normal secretion. Although the many factors affecting parotid secretion were not all accounted for, the evidence seemed to suggest reduction of the flow at such times. Following this suggestion an investigation was undertaken involving a series of experiments in which a number of subjects worked on a variety of assignments requiring various degrees of mental application. Variations in the rate of secretion were carefully noted during these activities. The following report is a presentation of the results obtained.

*Accepted for publication September 1, 1930. The investigation upon which this article is based was supported by a grant from the Heckscher Foundation for the Advancement of Research, established by August Heckscher at Cornell University.

[1]B. Brunacci and E. T. de Sanctis, Sulla funzione secretoria della parotide nell'uomo, Nota II: Influenza inibitrice dell' attivita psichica sulla quantita e qualita della saliva secreta, *Arch. di fisiol.*, 12, 1914, 441-454.

[2]K. S. Lashley Reflex secretion of the human parotid gland, *J. Exper. Psychol.*, 1, 1916, 461-493.

Method and Procedure

The method of collecting and measuring the secretion was the same as that used in previous investigations, therefore it need not be described again.[2] This technique would not interfere in any way with effective mental effort. After the suction disk had been securely fastened over either the right or left Stensen's duct the general procedure was to allow *S* to sit quietly at a table and await further instructions. He was told to keep his mouth closed and to avoid needless lingual and swallowing movements. Variation in the normal flow of secretion served as a basis of control for most of the experiments, although some tests were conducted when the glands were under active extero-stimulation. The rate of secretion was recorded in hundredths of a cubic centimeter at the end of every minute. The amount collected for a definite period of repose either before or after the activity period served as a control flow and is so designated in the figures.

The *S*s for most of the experiments reported were male and female adults of both graduate and undergraduate standing who were paid for their time. Preliminary tests were made with every *S* in order that he should become familiar with the apparatus and eliminate any anxiety as to the nature of the work to be done. To insure mental application, the problems provided consisted of mechanical puzzles of various types, mirror drawing, threading needles, adding columns of figures, multiplying two-place numbers without mechanical aid, crossing out vowels in printed material and taking mental and achievement tests.

Results

Influence of mental activity. In the light of the conflicting results of the studies cited above, the first determination to be made must of necessity deal with the general nature of the influence of mental activity on parotid secretion. Many tests were made with every *S* during the types of activity listed above and without exception the secretion was reduced whenever *S* was engaged with a problem requiring a more or less intense mental effort. The extent and duration of this inhibition varied for different *S*s and for the same *S* at different times; but in no case have we found mental work to have an excitatory effect on the normal rate of secretion. Forty records might have been selected to show this phenomenon but in the interest of conservation of space we shall present but one typical record.

Fig. 1 represents the quantity of saliva from one parotid gland when *S* was resting and working during alternating 3-min. intervals.

The activity consisted of tracing a six-pointed star seen only in a mirror. After the control flow had been checked *S* was instructed to take a copper stylus lying on the table in front of him and, guided by the reflection in the mirror, follow a glass pathway arranged in the shape of a star. This pathway, about 3 mm. in

[2] A. L. Winsor, Factors indirectly affecting parotid secretion, *J. Exper. Psychol.*, 13, 1930, 423-437.

width, was cut out of copper sheeting which together with the stylus was in circuit with an electric counter. So long as *S* guided his stylus on the glass no contacts were made, but if the stylus touched either side a contact was recorded and counted as an error. The sides of the copper plating were notched to prevent the stylus being guided along an edge. *S* was instructed to avoid contacts and continue tracing the star until the signal to stop was given. Both the number of contacts and the number of circuits or parts of a circuit were recorded. That such an assignment requires close attention will be evident to anyone who has attempted a similar performance. After 3 min. of such activity *S* was signalled to stop and remain quiet. At the close of a 3-min. rest-interval the mirror drawing

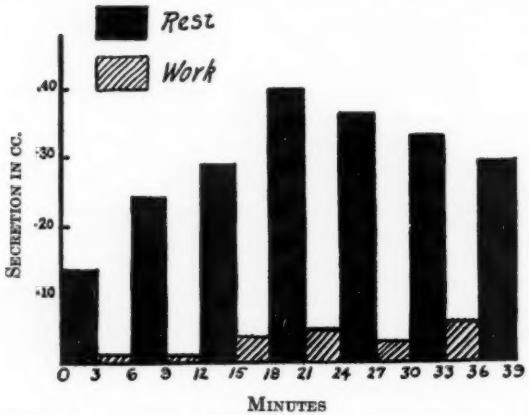


FIG. 1. EFFECT OF MIRROR DRAWING ON GLANDULAR (PAROTID) ACTIVITY (Total quantity secreted during rest periods 2.04 cc., during work periods 0.20 cc.)

was resumed and subsequent activity alternated with periods of rest for 42 min. The quantity of secretion collected for every one of these periods is shown in the figure. The striped columns represent this secretion collected during the activity periods and the solid columns during rest.

It will be noted that there is a marked reduction of secretion during the active periods and that this reduction is greater during the early intervals than during the later ones. Successive rest periods also show progressive increase for the first few intervals. These data support Brunacci and De Sanctis in indicating salivary inhibition during mental effort. This phenomenon will be further demonstrated in the figures that follow.

Duration of inhibition. Assuming that the reduced rate of normal secretion is due to inhibition either directly or indirectly the next

problem to suggest itself relates to its possible duration when no rest-periods are interposed.

Tasks were arranged which could be continued for a protracted period without pause and the secretion for successive 5-min. periods was measured. As in the previous experiments, a 5-min. period before and after the assigned work served as a control. The *Ss* (adults) were not aware of the nature of the experiment but were instructed to continue the activity until told to stop. The test in geography consisted of combining the separate states of the United States in their proper

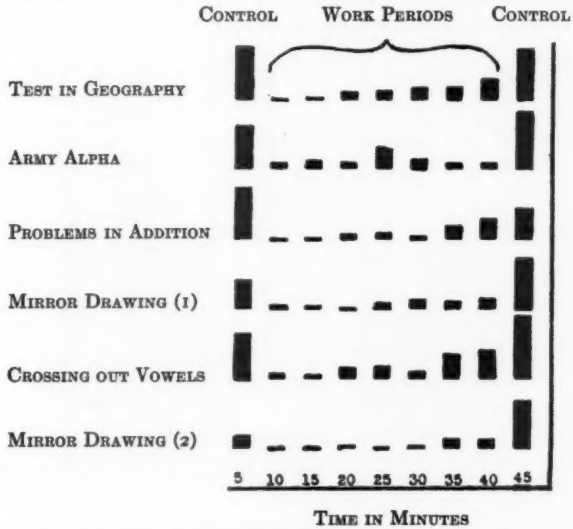


Fig. 2. Effect of Prolonged Mental Activity on Secretion (Secretion in cc. for consecutive 5-min. periods)

positions as quickly as possible. For the mental test *S* was assigned all but Test I of the Army Alpha and allowed to proceed for from 3 to 6 min. on a test before going to the next. The problems in addition consisted of a series of problems of 5 columns with 6 digits in each column. The mirror drawing was as described above; crossing out all of the vowels in a closely printed series of words as rapidly as possible constituted another assignment.

Examination of Fig. 2 shows again the tendency toward inhibition during mental activity with variation in the extent of the inhibition as the activity continued. Some degree of inhibition appears to obtain throughout the 35 min. of each of these problems and in general the secretion is less at the beginning than toward the end of the

activity. In some cases the release of inhibition appears to be progressive after the first 10 or 15 min. Exceptions to this general tendency, such as the record of the fourth period during the mental test assignment, are due to a temporary cessation of activity. In this case *S* stopped to adjust his papers. It would seem that any mental activity requiring effort is accompanied by inhibition and that the duration of the inhibition depends somewhat on the nature of the

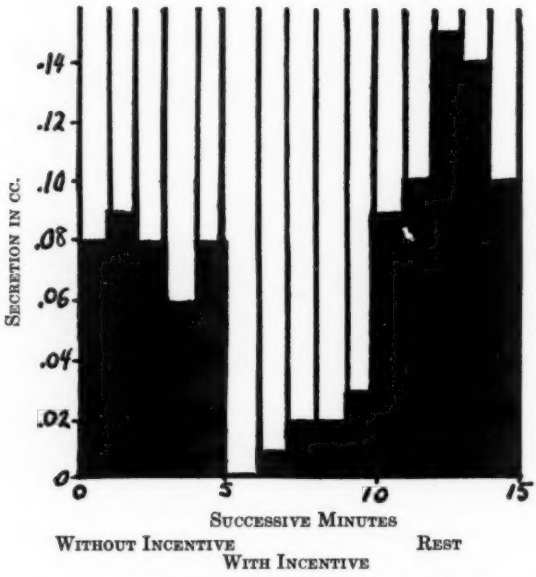


Fig. 3. Effect of Incentives on Secretion

activity. It is interesting to note that inhibition continued to some extent during the very fatiguing process of continuing the star-tracing for 35 min. The *S*s became restless and these frequent shifts of position would be expected to affect the inhibition more than it did. Such activity as mirror drawing during the *S*'s first experience with it provides much more complete inhibition than the semi-automatic activity of crossing out vowels. After long experience with mirror drawing it too becomes more or less automatic and the degree of inhibition is not so great. This will be shown in Fig. 3.

Effect of incentives on inhibition. In any steady work produced without particular attention, the subsequent adaptations of the various organs of the body are probably automatic, the central reinforcing impulses being of especial importance when the organism has to put forth its maximal effort. The ability of the organism to control glandular functioning indirectly through mental effort may be illustrated by the introduction of a strong incentive to effort when the *S* is proceeding more or less automatically at his task.

After a 13-yr. old boy had been doing the mirror drawing exercise described above several times a week for 3 mo. the process became decidedly boresome. He performed the task when assigned because he was being paid, but he frequently protested its monotony. His activity under these conditions became automatic and the inhibition which characterized his earlier performance disappeared. Our purpose had been to develop just such an attitude in order that we might observe the effect of incentives of various sorts.

After having recorded the secretion and errors over a 5-min. period of more or less automatic functioning, *S* was told that beginning with the next round he would be given 5 cents every time he went around the star making 10 errors or less.

The secretion and errors for successive minutes under the incentive are shown in Fig. 3. The final 5-min. period represents the secretion during rest. This new incentive to greater effort apparently is followed by a definite reduction in secretion. The automatic activity appears to be replaced by carefully controlled activity which results in a distinct reduction of errors. Apparently we have here an objective method for determination of the effect as well as the relative value of various incentives to mental work. From what is known of the significance of close attention in mental activity it would be expected that these periods of intense inhibition would also represent periods of highest efficiency. The following preliminary investigation was an attempt to ascertain if such a correlation obtained.

Relation of inhibition to efficiency. To determine the relationship between efficiency and inhibition, tasks permitting some degree of measurement of efficiency were assigned while variations in secretion were recorded.

Assuming that the elimination of errors on the star-tracing device cited above while proceeding at a reasonable speed represents efficiency, an *S* who had had long experience with this device was instructed to continue to go around the star with his stylus until told to stop. No further instruction was given. As he continued this activity for nearly an hour, the number of circuits made divided by the contacts for consecutive 5-min. periods was checked against the secretion for the same periods.

The results are shown in Fig. 4. The solid columns represent the secretion and the stippled columns represent the relative efficiency. A comparative analysis will show that in general when the secretion was low the efficiency was high. In other cases where computation was assigned it was found that when the *S* did more problems correctly there was a greater reduction in secretion than when he did the same number of problems only partially correctly or did a greater number a

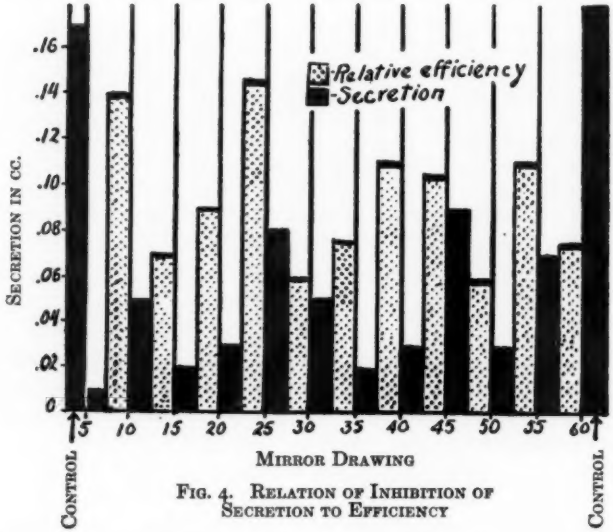


FIG. 4. RELATION OF INHIBITION OF SECRETION TO EFFICIENCY

large portion of which were incorrect. These data suggest a positive correlation between organic efficiency and intensity of secretory inhibition.

Conditioned response and inhibition. Up to this point our data have dealt with the inhibition of what appears to be unconditioned reflexes. Whatever the source of stimulation of the normal flow, it is undoubtedly not an acquired reaction. We were interested in observing the effect of an assigned mental problem on a conditioned salivary response. Just before completing her work as subject in the laboratory each day one *S* was given a bar of candy and had come to expect it. On the day on which the record to be presented was made the *S* had completed the day's work and had been seated at the table as usual for 5 min. awaiting further instructions. The secretion for every minute was recorded. At the end of that time a bar of candy was placed on the table with the instruction that she was to delay eating it until given a signal. The secretion was recorded during a

5-min. interval and then, with the bar of candy still before her, *S* was given a series of two-place numbers to add without the aid of a pencil. This continued for 5 min. during which correct answers were given to all of the problems attempted.

Fig. 5 shows the rate of secretion for the 5-min. preceding the appearance of the candy and the rate during the 10 min. that the candy was on the table both before and during the computation. As

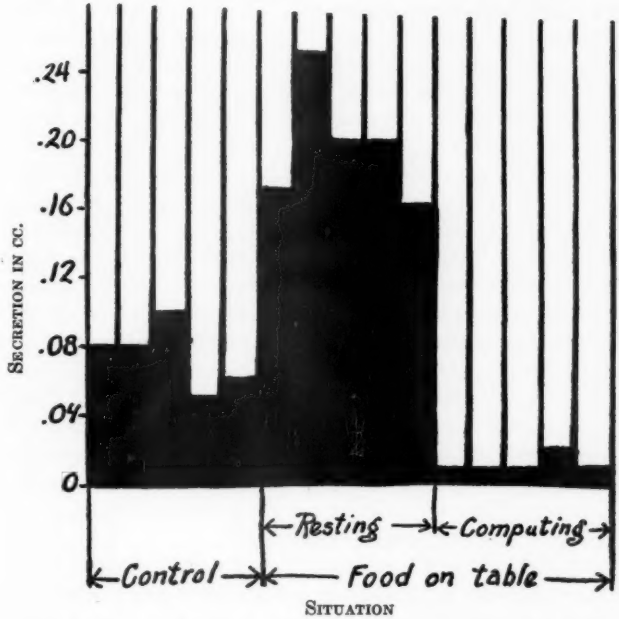


Fig. 5. Effect of Mental Effort on a Conditional Response

would be expected, the conditioned response like the unconditioned response is inhibited during mental effort. This demonstration emphasizes the difficulties one must expect in attempting to apply the conditioned reflex technique of Pavlov to human *Ss*.[4] The disturbing effect of his "investigatory reflex" on all conditioning experiments with dogs will be recalled. Apparently the center which controls the conditioned response to food is readily dominated or inhibited by the centers involved in computing.

[4] I. P. Pavlov, *Conditioned Reflexes*, (G. V. Anrep, tr.), 1927, 16.

Discussion and Conclusion

This preliminary survey of the effect of mental effort on the rate of parotid secretion indicates a definite reduction of free secretion during more or less intense mental application. The extent of the reduction appears to depend to some degree upon the type of mental activity employed, the duration of the process, the intensity of the effort, and other factors not yet investigated, such as the maturity and general stability of the *S*. This diminution in the normal secretion during an attentional state appears to be a fundamental aspect of organic adjustment. To explain and evaluate the significance of this reciprocal relationship it is necessary to determine the source of stimulation of this free flow.

Before considering the source of stimulation, however, it appears to be necessary to verify its existence. Nowhere in the extensive literature dealing with the salivary process in animals is consideration of this factor to be found. Apparently secretion does not flow from the glands of animals such as dogs when they are under the type of control used in Pavlov's laboratory. Furthermore, three of the more extensive investigators of salivary secretion in human *Ss*—Mitscherlich,[5] Zebrowski,[6] and Brunacci and De Sanctis[7]—noted no secretion from the parotid during repose. The first two of these investigators, however, were using *Ss* with fistulas resulting from cuts or burns in childhood and the latter was using a small cannula. Krasnogorski has shown that secretion from such abnormal fistulas is considerably less than from the normal duct.[8] A cannula placed in the mouth of an easily expanded duct would probably not collect accurately enough to show this small flow. Lashley, using his salivary disk to collect the secretion, was apparently the first worker to observe the continuous flow in the absence of marked extero-stimulation. The possibility that his method of collecting was responsible for the secretion was carefully considered and discounted by this author. His reasons for excluding this mechanical stimulation as an exciting agent were three, (1) "The presence of a large glass rod or

[5]C. G. Mitscherlich, Über den Speichel der Menschen, *Pogg. Ann. d. Phys Chem.*, 27, 1833, 320-345.

[6]E. von Zebrowski, Zur Frage der sekretorischen Funktion der Parotis beim Menschen, *Arch. f. d. ges. Physiol.*, 110, 1905, 105-173.

[7]Brunacci and De Sanctis, *op. cit.*

[8]N. Krasnogorski, Ueber die Wirkungen mechanischer und chemischer Reizungen verschiedener Teile der Mundhöhle auf die Tätigkeit der Speicheldrüsen bei Kindern, *Jahrb. f. Kinderheilk.*, 114, 1926, 268-276.

other tasteless object in the cheek with the drainage tube does not increase the rate of secretion. (2) Mechanical stimulation of the cheek and gums, where not covered by the drainage tube, does not call out reflex secretion until it becomes strong enough to excite reflex avoiding movements. (3) Manipulation of the drainage tube after adjustment, while it may force out a few drops of secretion from the receiving chamber, does not increase the rate of secretion as measured in successive minutes. From this evidence it seems safe to conclude that the secretion collected from the drainage tube represents fairly the normal secretion of the gland."[9] Additional support for this conclusion is found in the fact that when the *S* is lying on his back with the musculature of the body in a state of relaxation the rate of secretion is greatly reduced. When one *S* in our laboratory sat quietly at a desk inhibiting tongue and throat actively for one hour the secretion was 1.80 cc. For the next hour during which he was lying on his back the secretion was 0.20 cc. If the disk were the source of stimulation this marked difference in the two postural situations should not obtain.

If the existence of such a flow is granted the next problem is to locate the source of stimulation. The variation in the quantity of the secretion during postural changes affords our only suggestive clue. Buff noted the effect of movements of the body in exciting secretion in dogs.[10] Lashley reported that movements of the body seemed to have a slight stimulating effect and in a footnote suggested the possibility "that the apparent excitatory effect of shifting the position really acts by removing the inhibitory effect of muscular tension."[11]

A more logical and direct explanation for which we have found limited support is that the excitatory effect is a result of vascular tone. Tonus is generally ascribed to the automatic activity of the muscular fibers themselves and the vessels of the body are in turn kept in a state of tonic contraction by impulses arising in the vasomotor center. When the human subject is sitting or standing the musculature involved in the postural reflexes would therefore cause increased tone with consequent constriction of the vessels and a rise in blood pressure. Such a condition of affairs would be expected to

[9]Lashley, *op. cit.*, 464.

[10]R. Buff, Revision der Lehre von der reflectorischen Speichelsecretion, *Beitr. z. Anat. u. Physiol.*, 12, 1888, 1-39.

[11]Lashley, *op. cit.*, 490.

inhibit rather than increase secretion since glandular activity is dependent upon an adequate blood supply. In order to get an increased blood supply under these conditions it would be necessary to inhibit this tone by an inhibition of the normal constrictor impulses to the glands. In many parts of the body there is evidence of the existence of a nerve supply to blood vessels antagonistic in its function to the vasoconstrictors. Such a nerve is the small petrosal nerve of the parotid gland.

Hence while increased tonicity characterizes the organism as a whole during these postural attitudes this antagonistic nerve supply to the parotid gland would cause a dilatation of the vessels of the gland. Dilatation of these vessels as has been shown in dogs is accompanied by an increased blood supply in the gland and a flow of free secretion from the duct. These facts seem sufficient to account for the small normal flow of secretion from the human subject when large areas of the musculature are active.

This explanation would account for the variation in secretion observed during changes in posture and during changes in the degree of physical exercise. It would account for the inhibition of flow when the subject reclines and for the fact that man has such a free flow of saliva while the dog standing quietly has none. Furthermore the general characteristics of this response agree with what is known of postural tone reflexes. Skeletal muscular tone is habitually feeble in action, according to Sherrington. "Of all reflexes it is the tonic reflexes, *e.g.* of ordinary posture, that are in my experience the most easily interrupted by other reflexes. One great function of the tonic reflexes is to maintain habitual attitudes and postures. They form therefore a nervous background of active equilibrium. It is of obvious advantage that this equilibrium should be easily upset, so that the animal may respond agilely to the passing events that break upon it as undercurrent stimuli."[12]

Assuming the probability of this explanation of the free secretion, the next problem is to account for its inhibition during mental effort as shown in the figures. Although mild muscular activity is accompanied by secretion, attention should be called to the fact shown by Lashley and confirmed by us that there is an active inhibition of secretions during violent muscular effort.[13] We have also

[12] C. S. Sherrington, *The Integrative Action of the Nervous System*, 1923, 231.

[13] A. L. Winsor, Conditions affecting human parotid secretion, *J. Exper. Psychol.*, 11, 1928, 355-363.

shown that states of anxiety, such as the anticipation of an electric shock, cause inhibition of this flow. Whenever the organism is called upon to adapt itself to a situation involving either motor or emotional strain the vegetative processes of the body are temporarily inhibited. Through the process of reciprocal innervation the functioning of a part is sacrificed to the welfare of the whole organism. All parts of the body must coöperate in subordination to the activity of that tissue whose function during the emergency is of greatest importance. The outstanding feature of the central nervous system is the dominance of the cerebrum. Since effort in motor and emotional processes is characterized by reduction of secretion it would be expected therefore that conscious processes constituting the chief inhibiting mechanism of the body when actively at work would also make demands on the energy of the organism which would warrant temporary inhibition of the digestive mechanism.

Evidence of this focusing of the energy of the body on the more active tissues during an attentional state is a commonplace observation. The attentive person, as everyone knows, ignores ordinary sensory impulses and becomes more or less immobilized. Fearing has shown that attentional control reduced the amount of body sway in statistically significant amounts.[14] Just as shuffling of the feet and stretching of the arms are temporarily inhibited during mental effort, so it appears is the free flow of saliva temporarily inhibited. This inhibition of motor activity appears to be closely associated with, if not directly responsible for, the inhibition of the saliva. As the blood supply is needed for the heart, the lungs, and the brain during excitement it appears to be allocated to these areas also during mental activity. In the interest of organic efficiency the splanchnic function gives way to the somatic function.

This must not be interpreted to mean that the direct salivary reflexes from the oral cavity are inhibited during mental effort. If one were to swallow or chew during computation the secretion would be evoked. The relationship of the direct reflex to mental activity must await further research.

The importance of this technique as a basis for analyzing the relationship of the cerebrospinal to the sympathetic process is apparent. Heretofore such studies have been limited largely to the lower forms of life, except for the extensive use of the patellar reflex

[14] F. S. Fearing, Factors influencing static equilibrium, *J. Comp. Psychol.*, 5, 1925, 1-24.

in tonicity studies in man. Such a control as we have described here permits of a quantitative analysis of sympathetic activity during all phases of human behavior.

Summary

(1) In a preliminary survey of the effect of mental effort on the rate of parotid secretion the evidence indicates a definite reduction of normal secretion during a more or less intense attentional state.

(2) The extent of this inhibition seems to depend among other things upon the type, duration, and intensity of the mental activity employed.

(3) The introduction of an incentive to effort is accompanied by a pronounced decrease in the salivary flow.

(4) There appears to be a high correlation between organic efficiency and the extent of the inhibition.

(5) Conditioned salivary responses are readily inhibited when the subject undertakes a problem in computation.

(6) Evidence is presented indicating that the free flow of saliva used as a control in these experiments is due to the excitatory effect of vascular tone.

(7) This being the case, the inhibition of this reflex noted during mental effort is to be explained in terms of the restraining influence of the cerebral cortex. Voluntary activity is superimposed upon and inhibits postural tonicity.

(8) The technique employed offers a new approach to a study of the basic process of adaptation.

THE INFLUENCE OF ADRENAL EXTRACT ON BEHAVIOR AND MENTAL EFFICIENCY

By Arthur T. Jersild, Columbia University, and William S. Thomas, M.D., St. Luke's Hospital, New York

What is the effect of epinephrine on mental efficiency? This question, which has doubtless suggested itself to many who read the work of Cannon and others on the rôle of the adrenal glands in emotional excitement, is the subject of the present study.

The theory of the "emergency" function of the adrenal glands in emotional excitement, as proposed by Cannon, is based upon findings which may be reviewed somewhat as follows. When an animal experiences an exciting emotion, epinephrine is discharged in augmented amounts into the blood stream. Changes result which cause an increase in physical capacity. The pulse is accelerated; the blood pressure rises; glucose stored in the liver is released into circulation; muscular strength is increased at the expense of decreased activity of the digestive tract; blood coagulates more quickly; there is augmented resistance to infection, and fatigue is lessened; the bronchial tubes dilate, while perspiration, erection of the hair, dilatation of the pupils and trembling of the limbs may occur. According to the "emergency" theory, these changes mobilize energies needed for combat or flight in the animal's struggle for existence.

This physiological mechanism, adapted to the exigencies of primitive struggle, is still retained, it appears, by the present-day human being. Even though the struggle for survival in modern society depends more upon mental alertness than upon muscular strength, the human being continues to show widespread physiological changes when confronted with a trying situation. On the occasions for anger and fear, when clear thinking helps more than strong fists, the individual still exhibits trembling hands and a racing pulse. The utility of these symptoms in civilized human beings might well be questioned.

Quite apart from any theory of the emotions or of the causal relationship between emotional excitement and the functions of the

*Accepted for publication March 22, 1931.

adrenal glands, it is known that artificial administration of adrenal extract gives rise to symptoms similar to many of the changes exhibited in strong emotion. The way is thus opened for an indirect study of the question as to whether the bodily changes in strong emotion aid or interfere with the exercise of mental ability. In the present experiment, adrenal extract was administered hypodermically for the purpose of studying its effect on (a) mental performance, as compared with (b) motor performance, (c) physiological response, and (d) emotional behavior.

Procedure

A preliminary period was devoted to practice of the mental and motor performances employed in the study. In the experiment proper, the subjects were tested on a given day when no drug was given, on a following day when an injection of adrenalin chloride was administered, again, on a day without the drug, and so on throughout the course of the experiment, except when the routine was interrupted by an injection of saline solution for control purposes. A comparison of the measurements obtained when adrenalin was given with those obtained on normal parallel days and those obtained on the occasion of the injection of saline solution provides the data of this study. Two motor and six mental and three physiological tests were used.

Motor tests. The motor performances included:

(*1*) *Strength.* The Smedley hand dynamometer was employed to test the strength of grip. Fifteen consecutive trials were recorded at each administration of this test.

(*2*) *Tapping.* The customary tapping apparatus, designed to measure speed of motor coördination by means of an electric counter which registers each contact made by a hand stylus against a metal plate, was used in this test. Three successive 1-min. tests were taken on each occasion.

Mental tests. The mental tests were chosen to cover elementary activities in the field of learning, free association, calculation, and perceptual-verbal activity. The specific tests, all of which were measured in terms of speed, included:

(*1*) *Substitution.* The Woodworth-Wells blank, containing twenty unpatterned repetitions of each of five geometric forms, was used. The usual key at the top of the sheet, indicating which number is to be written into each of the forms below, was so modified as to provide eight separate keys. The keys were rotated in such a manner as to provide equal conditions for adrenalin and non-adrenalin days.

(*2*) *Free association.* Each test consisted of fifty stimulus words, presented to the subject with directions to respond to each serial word with the first word that came to his mind.

(*3*), (*4*), (*5*). *Calculation.* Three calculation tests were used, all of them based on the Woodworth-Wells calculation blanks comprising 100 two-place numbers arranged in four columns. Separate time scores were taken for the first two and the last two columns.

(a) Add 14. The task in this test was to add 14 mentally to each serial two-place number and to give the answer orally.

(b) Subtract 7. Procedure as above.

(c) Add the two digits. This test called for rapid writing of the sum of the digits of each of the two-place numbers.

(6) *Color naming.* The Woodworth-Wells color naming blank, containing twenty unpatterned repetitions of each of five colors, and scored in terms of the time required in naming the 100 colors, was used.

Physiological tests. Three measurements of the physiological effects of the drug were made each time.

(1) *Pulse rate.* (a) The pulse rate was taken before the dose was given; (b) after the dose had begun to take effect and immediately prior to the administrations of the mental and motor tests; and (c) at the end of each experimental period.

(2) *Blood-pressure.* Systolic and diastolic readings were made by means of a Baum sphygmomanometer and stethoscope in accordance with the procedure used in taking the pulse. Pulse pressure, which represents the difference between systolic and diastolic blood pressure, was calculated from these readings.

(3) *Analysis of urine.* The urine of every *S* was analyzed for sugar when sampled under normal conditions and after administration of the saline solution. Samples of urine were taken on the first occasion for micturition following the experimental period in which adrenal extract had been given. The usual Benedict's solution color test was employed to determine the presence of glucose.

Other observations. In addition to the measurement of the physiological effects of the drug, a record was made of other rather obvious symptoms which could not be recorded in quantitative terms. Among these symptoms were trembling of the hands, and sometimes of the entire body, pallor, restlessness, emotional irritability, and complaints of subsequent drowsiness and fatigue.

Subjects. Six persons took part in the experiment. One of these was eliminated when it became impossible to take adequate tests by reason of the violent trembling occasioned by the drug. Three *S*s completed a series of 6 adrenalin and 6 non-adrenalin days, and 2 *S*s completed a similar series of 4 days each. The *S*s, 2 men and 4 women, ranged in age from 21 to 28 yr., and were all above the average in mentality.

Dosage. The preparation employed was a 1-1000 solution of adrenalin chloride, injected into the fleshy part of the upper arm or the thigh. The size of the dose ranged from 7 to 16 minims, depending upon the susceptibility of the *S*. The adrenalin was injected in sufficient quantities to effect an obvious reaction as shown by pallor, tremor, pulse rate, and blood pressure. If an injection failed to produce unmistakable symptoms, an additional quantity was administered to bring about the required change. For control purposes, the *S*s were given an injection of saline solution instead of adrenalin on a regular experimental day. The *S*s had no intimation beforehand of the change from the usual dosage, and there was no deviation from the usual procedure.

Schedule. Since individuals differ substantially not only in their susceptibility to the drug but also in the length of time that elapses before the drug takes effect, a constant time schedule could not be applied to all the *S*s. The mental and motor tests, which required an average of 30 min., were not begun until definite physiological symptoms set in after the injection of adrenalin. The experiments took place late in the forenoon.

RESULTS

In presenting the results, attention will be given first to physiological reaction and changes in behavior effected by the adrenalin, and secondly to the effect of adrenalin on the mental and motor performances.

General symptoms. The physiological and behavior changes caused by adrenalin covered a wide variety of conditions, and revealed many individual differences. Trembling of the hands was usually the most noticeable sign. It occurred in the case of all the *Ss*, and sometimes involved the entire body. All the *Ss* reported what may be called "internal trembling," This condition was sometimes described as a "tingling" or "quivering sensation" in the trunk or extremities. Four *Ss* reported that they stuttered in naming the items of the color naming test. This "stutter" seemed to be entirely subjective, since no impediment of speech was apparent to the experimenter.

Coincident with a gradually accelerating pulse rate appeared awareness of the pulsations of the heart. Frequently an S was able to count his pulse correctly on the basis of cardiac sensations.

Varying degrees of pallor were observed in all the *Ss*. In connection with this symptom, as with all others, personal idiosyncrasies were apparent. A marked degree of pallor was not necessarily accompanied by a similar degree of tremor; the *S* who trembled and grew pale most quickly after the injection of adrenalin showed only an average change in pulse and blood pressure; the *S* who received the largest dose of adrenalin showed less pallor than the others, only an average change in blood pressure, while, on the other hand, glycosuria in this case was most marked. Individual differences could be noted independently of the size of the dose. One *S* exhibited almost immediate tremor and change in pulse, but these symptoms had begun to wear off by the end of 30 min. Another *S*, whose dose was one minim smaller, and who ranked second in the speed at which the drug began to take obvious effect, showed a still higher pulse rate and change in blood pressure after 40 min. The *S* whose normal blood pressure was highest showed the highest increase in pulse pressure when given a dose smaller than that given to any other.

Three *Ss* revealed greater emotional irritability under the influence of adrenalin. It appeared that provocations normally too mild to elicit public symptoms of anger were more effective when adrenalin had been given. Restlessness and frequent shifting about were often observed. One *S* had a tendency to uncontrolled giggling, and at one time the tests had to be interrupted while she made an effort to regain her composure. This *S* was urged by her friends to withdraw from the experiment because of her irritability later in the day after adrenalin had been given. Two *Ss* reported weeping spells

during the late afternoon or evening following injections. One individual described her mental state soon after adrenalin had been given as being similar to the aftermath of a period of intense fear. Dryness of the mouth, perspiration of the hands, and a feeling of being keyed up for action were also reported. One *S* complained of drowsiness and fatigue soon after the dose had been administered, while all reported a tendency to nervous fatigue, sometimes accompanied by headache, after the lapse of 1-3 hr.

Physiological measurements. Table I below presents the average readings for pulse rate, systolic and diastolic blood pressure, and pulse pressure when measured (a) immediately before the injection, (b) following the injection, but prior to the performance of the mental

TABLE I

AVERAGE READINGS WHEN ADRENALIN AND WHEN SALINE SOLUTION CONTROL WAS ADMINISTERED

		Pulse	Systolic	Diastolic	Pulse pressure
Adrenalin	Before injection	80	114	74.4	44
	After injection	90	122.5	61.	61
	End of experiment	89	126	60.8	67
Control	Before injection	79	111	71	40
	After injection	80	113	72	41
	End of experiment	75	113	70	44

and motor tests, and (c) at the end of the experimental period. Corresponding readings for the saline solution dose are given.

The above figures show an increase of 10 beats per minute in pulse rate, an 8.5 mm. rise in systolic blood pressure, a 12.6 mm. decline in diastolic blood pressure, and an increase of 17 mm. in pulse pressure when the average readings taken immediately before and soon after the injection of adrenalin are compared.

The fall in diastolic blood pressure, as indicated in the table, was greater than the rise in systolic. A decline in diastolic blood pressure following the injection was the most prominent physiological reaction exhibited in the present study.

A comparison of the effects of adrenalin and saline solution indicates that suggestion and the pain of the hypodermic needle were not important elements, so far as the *S*s in the present study were concerned. The saline solution was administered in the regular course of the experiment, but it occurred after the *S* had had experience on previous days with the needle and with the effects of adrenalin.

A reproduction of all the individual records would be necessary to illustrate individual differences in physiological response to the drug and also differences within the response of the same *S* on separate days. All the *S*s showed a distinct average rise in pulse pressure. The changes in pulse and blood pressure did not, however, exhibit a regular pattern. A sharp increase in pulse rate was sometimes accompanied by very little change in blood pressure. Likewise, a rise in pulse pressure was not always accompanied by an accelerated pulse

TABLE II

CASE STUDY OF EVERY *S*, SHOWING AVERAGE PULSE RATE AND BLOOD PRESSURE BEFORE AND AFTER ADMINISTRATION OF ADRENALIN, RANGE OF CHANGES FOLLOWING THE INJECTIONS, SIZE OF DOSAGE AND TIME OF APPEARANCE OF DISTINCT SYMPTOMS

S	Measurement	Before	After	Range	Dosage (minims)	Time (min.)
A	Systolic pressure	118	126	+18 to + 5	9	15
	Diastolic pressure	87	57	−20 to −10		
	Pulse rate	79	78	+ 6 to −14		
B	Systolic pressure	123	139	+26 to +2	8	10
	Diastolic pressure	75	59	−24 to + 4		
	Pulse rate	80	88	+20 to + 6		
C	Systolic pressure	111	115	+10 to − 6	7-9	22
	Diastolic pressure	75	70	− 8 to − 1		
	Pulse rate	82	91	+16 to − 3		
D	Systolic pressure	103	110	+20 to + 2	14-16	23
	Diastolic pressure	64	58	−12 to + 4		
	Pulse rate	83	95	+18 to + 6		
E	Systolic pressure	116	125	+14 to + 4	9	3
	Diastolic pressure	75	62	−22 to − 8		
	Pulse rate	75	91	+24 to + 8		

rate. In Table II below some of the differences in physiological response are shown.

The tests for glycosuria did not yield clear-cut results. No *S* exhibited sugar on normal days, or when saline solution was administered. One *S* showed a marked sugar reaction following the injection of adrenalin on one occasion. In the remaining *S*, glycosuria occurred on some occasions, but not invariably. So far as this experiment is concerned, it appears that marked changes in pulse and blood pressure occur more regularly in connection with such symptoms as pallor, tremor, and agitation than does glycosuria.

Mental and motor tests. The injection of adrenalin caused decreased ability to perform the substitution test, the test of adding

two digits, and of subtracting 7, in the case of all the *S*s. The scores attained in color naming, free association, and the test of adding 14 showed no consistent trend one way or the other. All the *S*s showed improvement in tapping ability and strength of grip when adrenalin had been administered.

Table III gives the average and relative scores of the mental and motor performances for the separate series of adrenalin and non-adrenalin tests. Standard deviations are given in parenthesis below each average. The averages are drawn from distributions representing the separate scores on each adrenalin and non-adrenalin series for all *S*s. Due to marked individual differences in strength of grip, the dynamometer readings showed a tri-modal distribution in both series. The standard deviations for this test are not given, since they are quite out of line with the records of the separate *S*s.

The scores for the two series reduced to a common denominator are given in the lower half of the table together with the reliability of the differences obtained. This 'reliability' shows the chances in 100 that the obtained difference represents a true difference greater than zero.[1] The reliability of the difference between the averages shown for the dynamometer is only approximate, since it represents the figure obtained when the two series of scores for all *S*s were reduced to a comparable series.[2]

The reader will recognize that records so limited in number as those of the present study do not lend themselves well to statistical treatment. Although it is true that the consistency of the trend shown by the records of the separate individuals was more impressive than the statistical reliability as shown in the table, the treatment of the data as presented in the table gives a good indication of the nature of the results, while it offers, at the same time, a caution against too liberal an interpretation of the data.

Trembling of the hands no doubt played a part in lowering the scores in the substitution test and the test of adding 2 digits, since both of these depend upon writing. In connection with the substitution test, however, there was not only a greater number of errors, which had to be equated in terms of time, but also evidence of greater dependence on the key when adrenalin was given. The time required in finishing the first half of the test conformed more closely to normal than did the time requirement of the second half. It was

[1] H. E. Garrett, *Statistics in Psychology and Education*, 1926, 128-133.
[2] *Ibid.*, 281-284.

TABLE III

SHOWING AVERAGE AND RELATIVE SCORES ON ADRENALIN AND NON-ADRENALIN DAYS AND THE RELIABILITY OF THE RELATIVE SCORES

Average Scores

		Calculation					Tapping			Dynamometer		
	Substitution	Add 2 digits	Subtract 7	Add 14	Color naming	Free association	(1)	(2)	(3)	(1st 3)	(7,8,9,)	(last 3 trials)
Adrenalin	123	31.7	71.7	85.5	47.7	57.8	440	428	424	49	43.3	42
S.D.	(24.6)	(5.9)	(20)	(25)	(9.6)	(8.4)	(21)	(21)	(21)			
Non-adrenalin	120	30.5	68	85.4	47.1	58.1	420	416	413	47	41.7	40
S.D.	(12.4)	(5.4)	(17)	(25)	(6.7)	(9.3)	(20)	(22)	(23)			

Relative Scores

Adrenalin	97	96	95	100−	99	100.5	105	103	103	104	104	105
Non-adrenalin	100	100	100	100	100	100	100	100	100	100	100	100
Reliability	76	80	79	nil	60	54	100	99	99	(86)	(86)	(86)

also observed that movements of the head and eyes were directed more frequently toward the key which accompanied the test when adrenalin had been administered than when the test was given under normal conditions.

The scores obtained when a saline solution control was administered corresponded so closely to those recorded for normal days that no significance attaches to them. In view of the negligible physiological effects of the control dose, the motor and mental test results are not reproduced.

Mention has been made of the feelings of nervous fatigue, sometimes accompanied by headache, which set in after the initial effects of the adrenalin had worn off. An effort was made to measure this condition by repeating the tests four hours after the regular experimental period. Only 3 *S*s were available for these subsequent tests. The data, so far as they go, do not reflect a falling off in efficiency commensurate with the reported feelings of fatigue.

The literature on the adrenal glands gives frequent mention of the effect of epinephrine in overcoming muscular fatigue. An attempt was made to test this factor in the present study. On one occasion the entire test series was repeated four times in immediate succession, yielding four records on each of the mental tests, a total of 12 one-minute tapping records, and 60 dynamometer readings. The records so obtained corresponded to the trend of the results under the usual procedure. The last of the four repetitions of the mental tests gave a higher score in the absence of adrenalin, while tapping and strength of grip scored higher under the influence of adrenalin.

Summary

The present study was designed to test the influence of adrenal extract on mental performance as compared with its effect on physiological response, general behavior, and motor performance. A preliminary practice period was followed by an experimental routine in which motor and mental tests were administered with and without a previous injection of adrenal extract on separate days. The preparation employed consisted of 1-1000 solution adrenalin chloride, administered hypodermically in dosages ranging from 7 to 16 minims, depending upon the susceptibility of the *S*s. An injection of saline solution was used for control purposes.

The physiological measurements showed an accelerated pulse rate, heightened systolic blood pressure, and a decrease in diastolic

blood pressure when adrenalin was administered. The decline in diastolic was more prominent than the rise in systolic blood pressure. The degree of change in pulse rate did not correspond consistently with the degree of change in blood pressure. Individual differences in the rapidity of the physiological response could be observed independently of the size of the dose. The control dose of saline solution had little effect.

The tests for glycosuria did not yield consistent findings corresponding to the severity of the circulatory effects or to the character of the overt behavior symptoms.

The observable behavior symptoms ranged from tremor, exhibited in varying degrees by all the *S*s, to signs of restlessness and emotional irritability. The *S*s reported "tingling" sensations, a feeling of "stuttering," and divergent affective experiences, such as feelings of strain, weeping spells, irascibility, and feelings of fatigue and drowsiness subsequent to the experimental period during which adrenalin had been administered.

The results of the tests of mental and motor performance showed that adrenalin did not favor mental efficiency, while the motor activities were somewhat improved by the drug.

IMPULSES FROM THE ACOUSTIC NERVE OF THE GUINEA PIG, RABBIT, AND RAT

By Ernest Glen Wever, Princeton University

The recent investigation of auditory nerve impulses by Bray and the writer,[1] in which the cat was used as the experimental animal, has been extended in the experiments here reported to three new species, the guinea pig, rabbit, and rat. These experiments were of an exploratory nature and limited in number, but are described here for the general information given on sensory responses in these animals, and because they add new checks to those previously reported relative to the fundamental findings.

Procedure

The basic procedure, like that previously described, involved the operative exposure of the auditory nerve, and detection of the impulses set up therein during stimulation of the ear by sound. The surgical technique, though fundamentally the same, varied in certain details in the different animals in accordance with differences in the anatomical situation.

The anesthetic (in all cases but one) was chloral hydrate, injected subcutaneously in 10% solution; sometimes in addition a small amount of ether was administered by cone to increase the depression.

Complete decerebration was satisfactorily performed with the guinea pigs, but in the other animals this was attended by a considerable loss of blood, and it was found that greater success could be achieved by leaving the cerebrum practically intact, and maintaining the animal in anesthesia. The injection of anesthetic had to be repeated only when the experiment was prolonged beyond four or five hours, as the effects of the first injection extended over about that period. With the rats a good deal of difficulty was experienced in getting the proper dosage of anesthetic.

The exposure of the acoustic nerve was practically as described elsewhere for the cat; it was rather difficult for the rat because of the small size of the animal.

*Accepted for publication September 5, 1930. From the Princeton Psychological Laboratory. The writer is much indebted to his wife for assistance in the experimentation.

[1] E. G. Wever and C. W. Bray, Action currents in the auditory nerve in response to acoustical stimulation, *Proc. Nat. Acad. Sci.*, 16, 1930, 344-350; and The nature of acoustic response: the relation between sound frequency and frequency of impulses in the auditory nerve, *J. Exper. Psychol.*, 13, 1930, 373 *ff.*

After exposure of the nerve, the usual hook electrode was applied, a second electrode embedded in adjacent inactive tissue, and the nerve impulses conducted through a vacuum tube amplifier to a telephone receiver. The results of stimulation of the ear of these animals by sound were essentially the same as those previously reported for the cat: tones, noises, and speech were translated into nerve impulses of frequency characteristics apparently identical with the original sound waves, and thus after amplification became audible in the receiver. The reception in most cases was a little below the average that has been established for the cat, in part no doubt because of poorer technique. The results with one of the guinea pigs, however, were as good as any previously obtained, and even a faint whisper was readily audible through the preparation.

Results

The results of the different experiments are given below in detail.

Experiment 1. Guinea pig 1. Decerebrated; left auditory nerve exposed, and electrode successfully applied.

Speech was received readily, and all tones from 250 to 5500 d.v. per sec.

Check: No responses were obtained when the active electrode was placed under the skin of the back.

Experiment 2. Guinea pig 2. Decerebrated; left auditory nerve exposed and electrode applied.

Speech, whispers, and all tones from 105 to 4000 d.v. were received.

Check: The right cochlea was destroyed by the following method. Working from the inside surface of the cranium, through an opening that had earlier been made exposing the apical (fourth), the third, and most of the second turns of the cochlea, by means of a fine dental drill a hole was bored at the third turn, and the destruction made practically complete by inserting a small spear needle into the drill-hole and pushing it laterally toward the base of the cochlea. After this destruction the reception was tested, and found to be almost as good as before; faint sounds, such as whispering, no longer came through, but responses were still obtainable to tones ranging from 125 to 3200 d.v.

Then through an opening that had been made exposing the vestibule and base of the cochlea on the left side, a small hole was drilled in the vestibule. The drilling was stopped as soon as the bone was penetrated, and though the drill-hole looked reddish, no bleeding or escape of fluid could be observed. Testing at this point revealed responses to speech and to tones between 125 and 500 d.v., but not to higher tones. The drill-hole was then penetrated and slightly enlarged with the spear needle; no profuse bleeding occurred, but the region became moist and somewhat bloody. Testing showed no responses to any auditory stimulus.

In the first, right cochlear, destruction probably, and in the second, left vestibular, destruction I should say certainly, no significant modification of the conductive system (drum, ossicles) could have resulted from the destruction operation. It is pertinent to note that the operative exposures of the two cochleas were made before the checking experiment began, indeed, even before the nerve was exposed. And though the three exposures involved six hours of continuous

operating, the time intervals between the testings mentioned above were of the order of ½ to 2 minutes.

The results of this experiment I regard as very powerfully in contradiction of a criticism recently received[2] that the results obtained by Bray and me are due not to nerve impulses but to condenser action of the drum and associated ossicles. Essentially the same experiment as described above has been performed on Cat No. 24, with like results.

Experiment 3. Rabbit 1. Common carotid arteries tied; decerebration. Bleeding from arterial circle profuse, and never completely checked. Nerve exposed and electrode applied just before breathing ceased.

Responses were received to tones of the middle range at first, but not after 10 min.

Experiment 4. Rabbit 2. Left temporal lobe of cerebrum removed; nerve exposed and electrode applied.

Responses were received to speech and all tones from 250 to 5150 d.v.

Check: An attempt to perform the cochlear destruction experiment mentioned above failed in this case, but supplied a creditable check none the less. The two cochlear regions were exposed by removing the neck of the mandible and working from below, but in the operation on the left side the drum was accidentally torn. Testing at this point showed responses still present, but after destruction of the right cochlea the responses ceased.

Experiment 5. Rat 1. Ether anesthesia; decerebration. Bleeding profuse. Nerve exposed and electrode applied, but the animal at this time was breathing only lightly.

Responses were obtained to tones between 500 and 4400 d.v., and faintly to speech. In about 10 min. responses were absent, and it was noticed that breathing had ceased.

Experiments 6 and 7. Rats 2 and 3. The first animal died immediately from an overdose of anesthetic (chloral hydrate), and the second from loss of blood following decerebration.

Experiment 8. Rat 4. Did not decerebrate; left lateral lobe of cerebellum removed, but nerve not exposed to view. Electrode applied blindly, but apparently successfully.

Responses were obtained to speech and tones between 250 and about 1000 d.v. There was possibly a faint response to tones up to 3000 d.v., but the observation was uncertain. The responses to speech were noticeably lacking in higher components. It may or may not be significant that this rat was very old.

Experiment 9. Rat 5. This animal died very soon from an overdose of anesthetic.

Experiment 10. Rat 6. Not decerebrated; right lateral lobe of cerebellum removed and electrode inserted in the right side of the medulla at the level of the internal auditory meatus.

Responses were received to speech and all tones from 275 to 4050 d.v.

[2]Personal communication.

Discussion

From the results given above it is plain that the range of tonal frequencies detectable by this method through the sensory apparatus and acoustic nerve of the three animals investigated is of the same general order as previously reported for the cat, namely, from around 200 or over 4000 d.v. per. sec. Here also, as in the cat experiments, there is no evidence that these values define the limits of frequency transmission by auditory apparatus or nerve in the animals; rather, they are restricted by the method of investigation, and higher amplification would doubtless reveal a greater frequency range.

In the limited results of these experiments it would be unsafe to look for any differences between the three species of animals used, for variations in the experimental technique, and perhaps in the acuity of the animals of each species, doubtless complicate the situation. Only extended experimentation on large groups of animals, which I am unable to undertake at this time, could be expected to exhibit any such differences.

The results for the rats are particularly disparate; consider Rat 4, with responses limited to low tones, as against Rat 1, with responses only to high tones. It is not clear whether we have to do here with age factors, middle ear or other anomalies, technical variations, or, more likely, some combination of these. It should be noted that the rats were all albinos, and this animal is notoriously subject to acoustic disorders, particularly affections of the middle ear.

The results of this investigation, while indicating that tonal frequencies of a fairly extensive range are transmitted through the sense-organ and peripheral nerve systems of the animals, do not of course give any evidence on the extent to which these frequencies pass the higher synapses and enter into the complex response patterns of the animals. It would be of interest to compare these results with precise determinations of the limits of tones to which these animals can be observed to react. Unfortunately, such determinations are lacking, though we have some scattered observations.

Preyer,[3] on the basis apparently of somewhat limited tests, reported that the guinea pig responds by a pinna reflex to tones from 1000 to 40,000 d.v. While others since Preyer have used this reflex as an indicator of hearing in the guinea pig, I am not aware of any

[3] W. Preyer, *Die Seele des Kindes*, 3rd ed., 1890, 72.

extensive repetition of his work.[4] Upton,[5] using a conditioned respiratory reaction as an indicator, has shown that guinea pigs respond to tones of 600 and 1000 d.v., but has not extended the method to ascertain the limits of frequency to which a response may be secured.

In the rabbit, Corbeille[6] has reported cardiac and respiratory changes in response to tones of 256, 2048, and 6500 d.v.

As regards the rat, the situation is puzzling indeed, for Hunter,[7] on the basis of extended investigations, limited however to the maze method, has reported that rats respond to noises but not at all to tones. While Watson in 1907[8] had observed that rats respond by bodily movements to the tones of the Galton whistle up to 17,000 d.v., Hunter, after repeating the experiments, concluded that the animals were responding not to the tone but to accompanying noise. However, in some recent experiments of my own to test this point, I have observed general bodily responses in the rat to tones around 10,000 d.v., and pinna reflexes up to 19,000 d.v., using a Pierce magneto-striction oscillator[9] that delivers tones which, though containing a small percentage of harmonics, are certainly noise-free. My observations, therefore, support Watson as against Hunter.

It is possible, of course, that an animal may give a simple reflex response to sounds which he may be unable to discriminate, or even

[4]Stepanow used the Preyer reflex, and reported that guinea pigs have an upper limit of hearing as high as man's. He found the reflex practically invariable with most sources of sound. Von Stein, however, found individual differences, with considerable variability in the response in some animals. Ewald, somewhat later, reported that "the range of hearing of guinea pigs is not materially different from that of man," but cited no experiments; I think it safe to say that his authority was Preyer. See E. M. Stepanow, Experimenteller Beitrag zur Frage über die Function der Schnecke, *Monatsschr. f. Ohrenheilk.*, 22, 1888, 85-92; Stanislaus von Stein, *Die Lehre von den Funktionen der einzelnen Theile des Ohrlabyrinths*, trans. from the original Russian by C. von Krzywicki, 1894; J. R. Ewald, Ueber die neuen Versuche, die Angriffsstellen der von Tönen ausgehenden Schallwellen im Ohr zu lokalisieren, *Arch. f. d. ges. Physiol.*, 131, 1910, 190.

[5]M. Upton, The auditory sensitivity of guinea pigs, this JOURNAL, 41, 1929, 412-421.

[6]C. Corbeille, L'influence des vibrations acoustiques sur la respiration chez la grenouille et certains mammifères, *C. r. Soc. biol.*, 101, 1929, 113-115, and L'influence des vibrations acoustiques sur le rythm cardiaque de lapins intacts ou décérébrés, *ibid.*, 563-564; also published in English, with E. J. Baldes, *Amer. J. Physiol.*, 88, 1929, 481-490, 491-494.

[7]W. S. Hunter, The auditory sensitivity of the white rat, and similar titles, *J. Animal Behav.*, 4, 1914, 215-222; *ibid.*, 5, 1915, 213-329; *Psychobiol.*, 1, 1918, 339-351; and *Ped. Sem.*, 34, 1927, 177-186.

[8]J. B. Watson, Kinesthetic and organic sensations, *Psychol. Monog.*, 8, 1907, (no. 33), 59.

[9]For a discussion of this type of instrument, see G. W. Pierce, Magneto-striction oscillators, *Proc. Amer. Acad. Arts and Sci.*, 63, 1928, 1-47.

to respond to in more complex bodily adjustments; and this may be a limitation of the reflex response methods if one is interested not in the sensory processes but in the extent to which the animal may make use of the stimuli. In this paper, however, it is the sensory mechanism that is concerned, and the reflex response data may be taken as significant, so far as they go.[10] Hence it is safe to point out that the two bodies of evidence, that from reflex responses to tonal stimuli and that from the acoustic nerve experiments of this paper, nothwithstanding the limitations of both, are in essential agreement in demonstrating in the animals investigated auditory sensitivity to sounds of a wide frequency range.

Summary

Auditory nerve impulses have been secured in response to sound stimulation in three new species, the guinea pig, rabbit, and rat. Some new checking experiments have been added to those previously reported, particularly an experiment showing the responses to disappear on destruction of the cochlea on one side and opening the vestibule on the other. The range of tonal frequencies to which responses have been obtained are fairly comparable in the different species.

[10]It seems fairly reasonable to say that the reflex responses to sound, such as Preyer's reflex, have an auditory basis, though the evidence is hardly as complete as one would wish. It has been shown that the reflexes disappear when the acoustic mechanism is radically interfered with; see von Stein, cited in footnote 5 above, and Robert Dreyfuss, Experimenteller Beitrag zur Lehre von den nichtakustischen Functionen des Ohrlabyrinths, *Arch. f. d. ges. Physiol.*, 81, 1900, 611.

THE REDINTEGRATION OF PLEASANT AND UNPLEASANT EXPERIENCES

By Ross Stagner, University of Wisconsin

This study attempts to investigate from a new angle the relation of emotional tone to memory. As usually phrased, this question runs as follows: What is the influence of emotional states upon memory? We prefer to restate the problem as Meltzer has suggested:[1] What is the relation of the feeling-tone aroused by an experience to the retention of that experience? This avoids a prejudgment of the causal relations involved. It is probably true that the factors influencing emotional reaction are likewise those which determine retention, rather than that the feeling-tone itself is a factor influencing memory.

The problem has been investigated in many ways: with questionaries; with pleasant and unpleasant odors; with words and other meaningless materials; and with the narration of personal experiences of the *S*. It is clear that the method last named is the only one suited for investigating the phenomenon which is, as Meltzer has pointed out,[2] so difficult to subject to laboratory test.

Kowalewski first used material from everyday life to investigate the thesis that unpleasant events are forgotten more rapidly than pleasant.[3] Just after a school holiday he had school children write down their pleasant and unpleasant experiences of the day before. Ten days later they were asked to recall as many of these as possible. He concluded that repression of the unpleasant was the rule, but omitted to correct his data for the fact that some events given the second time had not been given on the first recall.

Wohlgemuth also used the 'narrative' method with over 600 school children.[4] Judging from his reports, such *Ss* are not adapted for use in this kind of experimentation. His final data show that slightly more pleasant events were forgotten than unpleasant, but his method of treating the data is rather carefree, and his interest in disproving the psychoanalytic theory of forgetting may have influenced his technique.

*Accepted for publication September 1, 1930.

[1] H. Meltzer, The present status of experimental studies on the relationship of feeling to memory, *Psychol. Rev.*, 37, 1930, 124-139.

[2] *Loc. cit.*

[3] A. Kowalewski, *Schopenhauer und seine Weltanschauung*, 1908.

[4] A. Wohlgemuth, The influence of feeling on memory, *Brit. J. Psychol.*, 13, 1923, 405-429.

Meltzer used the same method with university students.[5] He found that a greater percent of unpleasant events were forgotten than pleasant, and suggests that the factors determining pleasantness or unpleasantness also determine the memory value of the experience.

Jersild employed the same technique,[6] and his results are comparable to those here presented. The author is in fundamental agreement with his discussion of explanatory factors.

There are several criticisms of the 'narrative' method which are of importance. First, a large number of events are used and on recall this may confuse the *S*. The 'span of attention' limits the individual's ability to recall in a given amount of time the events previously written which have no logical connections each to each. Secondly, the situation is not comparable with everyday existence, in which all sorts of cues are present to remind us of events we are trying to recall. The *S* is merely given a blank sheet of paper and asked to write from memory a number of events. Thirdly, the technique as ordinarily used does not control the recency or the intensity of the event. These considerations have led to the abandonment of this particular technique in the present experiment, in favor of another which is described below.

A few studies employing different techniques have appeared since Meltzer's review was written. Jones' repeated some earlier experiments of Smith[8] on the relation of galvanometric deflection to memory value. He found that certain words having a high deflection have a high memory value, but others have rather low values. He concludes that 'emotional' tone may influence memory in two ways—positively and negatively. He was not justified, however, in using the term 'emotional,' since the *S*s did not rate the feeling-tone of the words themselves.

Harden studied the effect of shock upon the recall of previously learned material.[9] For learning she used nonsense syllables, nouns, and three-place numbers. The situations interpolated between learning and recall were: reading, humorous story, shock, noise, and simultaneous shock, noise and loss of support. Only 10 *S*s were used, but each went through 15 sittings, combining each type of material with each situation. No significant losses occurred on recall. She suggests that the *S*s may have made an extra effort to offset the effects of the disturbance.

Würdemann persented data showing clearly the influence of intensity of feeling-tone upon memory.[10] He was not concerned with the problem of pleasantness and unpleasantness as such. His technique involved the association by the *S* of an odor with a form presented visually and tactually. The feeling-tone was rated on a scale of four, and very pronounced differences were found between those rated weak and very weak, and those rated strong and very strong. The number

[5]H. Meltzer, Individual differences in forgetting pleasant and unpleasant experiences, *J. Educ. Psychol.*, 21, 1930, 399-409.

[6]A. T. Jersild, Memory for the pleasant as compared with the unpleasant, *J. Exper. Psychol.*, 14, 1931, 284-288.

[7]H. E. Jones, Emotional factors in learning, *J. Gener. Psychol.*, 2, 1929, 263-271.

[8]W. W. Smith, *The Measurement of Emotion*, 1923, 29-44.

[9]L. M. Harden, The effect of emotional reaction upon retention, *J. Gener. Psychol.*, 3, 1930, 197-221.

[10]W. Würdemann, Über die Bedeutung des Gefühls für das Behalten und Erinnern, *Neue Psychol. Studien*, 1, 1926, 511-571.

of presentations of each combination and of recall tests seems to have been great enough to insure a high reliability, but no measures of variability have been calculated,—a fairly common source of difficulty with European studies.

Experimental Procedure

In every day life a great many cues are available that assist the *S* in the recollection of events and ideas. For this reason the technique used in earlier studies was discarded and the following one adopted.

Only a single event of each class, pleasant or unpleasant, was asked for from each *S*. Mimeographed sheets were used for the collection of the data. At the top of every sheet there was a space for name and date, and then the following instructions:

> Think of the most pleasant event which has happened to you within the past 15 days. There will be associated with that incident any number of trivial facts: an odor, the sound of a tune, the sight of a certain person, place, or object, etc. These associated facts probably will be objects or sense impressions occurring simultaneously with this event, although not necessarily. Please describe the incident briefly on the blank lines below; then, in the numbered spaces, enumerate as many associations as possible.

Beneath the instructions there were three blank lines for a description of the event, a space for the date of the event (number of days previously it had occurred), and then 10 numbered lines for associations. A similar sheet was used for the unpleasant events.

The sheets were collected as soon as they were filled out. None of the *S*s knew that a recall would later be asked for. A period of three weeks was allowed the *S*s for forgetting. During this interval *E* typed on new data sheets the information given by the *S*s, but omitted the list of associated items. In the recall series the *S*s were asked to fill in this list. The accuracy and completeness with which they did it was taken as a measure of the degree of redintegration of the experience. Scoring was on a basis of ideas rather than words. Associations which had not been given previously were thrown out. The result was converted into a percentage of the original number given.

The *S*s were sophomores and upper-classmen in the introductory and experimental psychology courses.[11] About 150 records were taken, but as a result of absences, inability to decipher signatures and various other factors, only 103 'pleasant' and 101 'unpleasant' records were obtained.

Results

The results of this study are summarized in Table I. The captions "No. Original Items" refers to the number of associations given on the original sheet; "Percent Recalled" to the percentage of these given correctly on the second recall; and "Recency," to the number of days previous to the original recall that the event itself occurred.

It will be seen, first, that the difference between the percentage recalled for P and U events is 11.0%, and that it is 3.37 times its

[11] I wish to thank Dr. R. W. Husband and Dr. A. T. Jersild for permission to use these classes for experimental purposes. The research was carried out under the direction of Dr. V. A. C. Henmon.

TABLE I

SHOWING THE EFFECT OF PLEASANTNESS AND UNPLEASANTNESS UPON THE NUMBER OF ASSOCIATIONS GIVEN IN THE FIRST REPORT, UPON THE PERCENT CORRECTLY RECALLED, AND UPON THE RECENCY OF THE ITEM ORIGINALLY DESCRIBED

	No. Original Items		Percent Recalled		Recency	
	P	U	P	U	P	U
Total	699	606	5610	4288	778	581
N	103	101	103	101	103	101
Average	6.8	6.0	54.4	43.4	7.6	5.8
S.D.	2.5	2.7	22.4	24.2	4.2	4.3
$S.D._M$	0.25	0.27	2.2	2.4	0.42	0.43
Diff. (P − U)	0.78		11.0		1.80	
$S.D._{Diff.}$	0.37		3.26		0.60	
$D/S.D._{Diff.}$	2.12		3.37		2.99	

standard error or 5.0 times its P.E. These figures approximate very closely to those obtained by Meltzer, who also used a three-week period of forgetting.

There is some evidence for an active process of repression going on even in the few days from the occurrence of the event to the first recall. The number of items associated with pleasant events averaged 6.78, while for the unpleasant events there were only 6.0. The reliability of the difference is only 2.12 (standard error equals 0.367) but the tendency is quite clear. The relative recency of the events lends more confirmation to this suggestion. The average pleasant event had occurred 7.6 days before the original recall, while the unpleasant events averaged only 5.86. The reliability of this difference is 2.99, which is for all practical purposes a statistically 'true' difference.

That the obtained difference in retention score is probably not as great as would be obtained if the events had been equal in recency may be inferred from these data. An average difference of almost two days in time would certainly affect the results to some extent.

The intensity factor is controlled in the present experiment by the use of only a single event from each feeling category. The instructions read "the most pleasant event" and "the most unpleasant event" in a given period of time. While chance, of course, might bring it about that a very intense event might be paired with a relatively weak one, this is fairly improbable and would be balanced out in so large a number of cases.

DISCUSSION

The above results seem to show fairly conclusively that, even when intensity is held constant, unpleasant feeling tone is associated with poor retention. It need not be mentioned that this is far from

being a rule, that it holds only within a large group under well-controlled conditions. It is true, on the other hand, that we are dealing with limited instances, and probable that in a more generous sampling of the affective life of the individual, if we were able to deal with it adequately, the trend of the present results would be more decisively shown.

It must nevertheless be emphasized that association does not under any circumstances imply causation. To say that the feeling tone influences retention is to enter a logical circle. There must be an appeal to some factor beyond the subjective. Such a factor may possibly be found in the nature of an adaptive response.

We are not concerned here to go into the nature of feeling. It seems reasonably obvious, however, that as an evolutionary product feeling must have had some biological significance. That non-adaptive responses are those which ordinarily call forth unpleasantness will be accepted as evident. Adaptiveness in a response implies the removal of a motivating tension or, as the psychoanalyst would put it, the satisfaction of an instinct. It is clear that in this sense adaptation would normally be accompanied by pleasure.

A sketchy examination of the unpleasant events cited by the subjects reveals that most of them involved setting up some kind of tension. Getting back a bad check, receiving a notice from the Dean, being criticized on a piece of work represent *things about which something must be done.* On the other hand, the pleasant situations usually involved a release from tension, or at least represented something complete, *e.g.* praise on work well-done, listening to a fine orchestra, seeing the latest movie. There is no tension set up. Nothing needs to be done about the situation.

This offers support to the theory that adaptation involves a release from tension.[12] It is clear that in most of the situations mentioned, failure to remove the tension will cause continued responses to the situation. These changes in the behavior pattern involve changes in the brain pattern, which must inevitably result in changes in the memory traces of the experience as related. On the other hand, the pleasant experiences involve no tension, there is no variability in response, and the memory trace remains unchanged.

Hull has recently attempted an analysis of variability of behavior in terms of the conditioned reflex.[13] He makes use of the empirically

[12]For this theory, see R. H. Wheeler, *The Science of Psychology*, 1929, 82.
[13]C. L. Hull, Simple trial-and-error learning, *Psychol. Rev.*, 37, 1930, 241-256.

determined fact that a reflex which results in satisfaction has its potency raised relatively to other reflexes conditioned to the same stimulus. It seems that the principle enunciated above may legitimately be suggested as supplementary to his; viz. that the inadequate response will be subject to the action of retroactive inhibition in consequence of the fact that it is followed by other responses, whereas the successful one is not. Thus learning may legitimately be said to be composed simultaneously of selecting the correct response and inhibiting the incorrect.

It has frequently been said that pure memory or recollection of personal experience differs fundamentally from habitual memory. It is hoped that the above analysis, such as it is, may show one more point in which they are essentially the same. If the principle of retroactive inhibition, which has usually been conceived as a phenomenon characteristic of habitual memory, can so appropriately be invoked to explain the forgetting of the unpleasant, there must be some relationship which is more than superficial. When both obey the same laws and behave in the same way, it is indeed difficult to conceive of them as fundamentally different.

Summary

(1) A technique is suggested by means of which the recall of pleasant and unpleasant experiences may be studied as redintegrative processes.

(2) Results obtained by this method show that there is a reliable difference between the completeness of redintegration of pleasant and unpleasant experience after three weeks.

(3) An active process of forgetting is suggested by the fact that there is a reliable difference between the relative recencies of the pleasant and unpleasant events given, and a considerable difference in the relative fluency of association of details for the two events.

(4) It is suggested that the more rapid forgetting of the unpleasant may be due to a principle fundamentally the same as that of retroactive inhibition.

THE PREDICTION OF AFFECTIVE VALUES

By J. P. Guilford, University of Nebraska

The present study is an attempt to predict the affective values of color combinations when the affective values of the individual components are known. By affective value is meant a position on the continuum which extends from greatest pleasantness to greatest unpleasantness. We shall assume in this discussion that the continuum is a single one, extending through an indifference point somewhere on the scale. In more concrete terms, the problem may be illustrated as follows: If a certain red has an affective value of 10, let us say, and a blue has a value of 4, can we predict what the value of a red-blue combination will be?

The problem is important from several points of view. If there is a law governing the combination of affects, we would be able to save a tremendous amount of labor in evaluating any number of color combinations for agreeableness or disagreeableness. Even the measurement of affective values of single colors is no small task when the method of paired comparisons is used, and this method seems to be the only good one for making such measurements. If we were to limit our task to 20 single colors and their combinations, taken two at a time, there would be 190 of the latter. To measure the affective values of 190 stimulus-combinations by the method of paired comparisons is no small undertaking.

The most important problem for theoretical psychology is that of the summability of affect. Given two discrete stimuli, each with its usual affective reaction, what happens when the two are presented simultaneously for affective judgments? Will there be a unitary *Totalgefühl*, as Wundt maintained,[1] an emergent totality with unique properties over and above those aroused by the component stimuli when given alone? Those who are mindful of the Gestalt psychology will think immediately of the dictum that the whole is more than the sum of its parts. Affective reactions to combinations of colors should, according to this school of psychology, be determined by the total configuration and not by its constituent parts. Or will the resultant feeling be an algebraic sum as Külpe,[2] Titchener,[3] and McDougall[4] have suggested? Or will it be a mean, a product, or some other mathematical function of the two components? The writer proposes to take the last mentioned views as working assumptions and also to try to determine to what extent they are true. The results of this study throw some interesting light upon the Gestalt hypothesis as applied to feelings. They do not settle the question, by any means, but they indicate that the constituent parts of the configuration are very strong determiners of the feeling-value of the whole. More refined methods may show that the parts have everything to do with the total result.

* Accepted for publication August 7, 1930.

[1] W. Wundt, *Physiologische Psychologie*, 5th ed., 1903, 341 ff.

[2] O. Külpe, *Outline of Psychology*, trans., 1909, 264.

[3] E. B. Titchener, *Textbook of Psychology*, 1909, 258.

[4] W. McDougall, *Body and Mind*, 3rd ed., 1915, 313.

In 1917 Geissler stated the law that the feeling-value of a combination of colors is proportional to the values of the components.[5] At that time, however, there was no accurate method of determining the feeling-values of stimuli, and he made no attempt to quantify his general law. The writer has at his disposal some data which not only verify Geissler's conclusion, but permit of quantitative predictions.[6] The data contain the affective values of 5 single colors as well as of the 10 combinations of the 5 colors taken 2 at a time. It has been possible to set up empirical equations from these data by which the value of the combination can be predicted from the values of the two components.

It is necessary to say something concerning the source of the data. The color stimuli were circular patches of light 5 in. in diam., projected from behind a ground glass screen by means of a device called a chromopathometer.[7] The sources of the lights consisted of the ordinary commercial "natural-colored" bulbs such as are used in advertising signs. The colors were very saturated. All stimuli, either the single colors or the combinations, were presented in pairs to be judged by the method of paired comparisons. The single lights were separated a distance of 44 in. The lights in the pairs were 29 in. apart and the distance between the members of the pairs was 2.5 in. When the pairs were exposed, the *Ss* were told to judge them as pairs or combinations. The fact that the judgment was to be based upon the combination was emphasized. A choice had to be made between only two things, which also favored unitary apprehension of the combinations. How many formed good configurations of the pairs and how many did not we do not know. Some *Ss* may have maintained an analytical attitude and thus kept the members apart. The conditions of the experiment strongly favored a consolidation of the pairs, both as regards instructions and as regards the spatial arrangement of the colors.

The *Ss* were students in general psychology. They were given the experiment in groups in a dimly lighted classroom. Altogether there were 460—249 men and 211 women. Fortunately, for the sake of verification, the *Ss* were used in three separate groups and one group was given the experiment a second time. This gives us the equivalent of 40 combinations and their two components, the affective values of which are known. In the experiment with the third group of *Ss*, the colored lights had been equated for intensity by the flicker method. This resulted in different affective values for some of the hues and their combinations. We will assume, however, that if any quantitative law exists between the feeling-values of the combinations and the corresponding values of their components, it should hold for one set of colors as well as for another whether the brightness factor is held constant or not. The data for the men and for the women are treated separately all the way through, because of the decided sex differences not only in the affective values themselves, but also in the laws of combination, as we shall see later.

[5]L. R. Geissler, The affective tone of color combinations, *Titchener Commemorative Volume*, 1917, 150-174.

[6]I am indebted to Mr. W. E. Walton for the use of these data. They are to be published in another journal at an early date.

[7]W. E. Walton and B. M. Morrison, The chromopathometer, *J. Exper. Psychol.*, 12, 1929, 254-258.

The affective values of the colors and of the combinations were computed by a method previously proposed by the writer.[8] This procedure has been justified empirically in another place.[9] The data which we are to use were not collected for this particular purpose and so they are not entirely satisfactory. Their only serious defect, however, is in the narrow range of values. The single colors have a maximum range from −0.800 to +0.610 for the men and from −0.736 to +0.562 for the women. The combinations have a maximum range from −0.850 to +0.519 for the men and −0.510 to +0.524 for the women. The unit of the scale values is the standard deviation of the normal distribution curve. The zero point of the scale is not the indifference point, but the mean of the scale values. The results therefore show a relatively narrow range of affective values, scarcely one j.n.d. (p.e.) on either side of the mean. This may be a serious matter, for any law which may be found to hold over the range from −1 p.e. to +1 p.e., may not apply beyond that range. We can at least see what the law of combination is within this range.

A reasonable preliminary guess as to the law would be that the value of the combination is proportional to the arithmetic mean of the values of its two components. From the ranges of affective values for the combinations, which are given above, the writer thinks it is clear that these values are not the algebraic sums of the values of the components. If this were true, the ranges would be much greater than those for the values of the single colors. If anything, the former are smaller than the latter. The means of the components of the 40 combinations were accordingly computed. These means were then correlated with the values of the combinations. For the men, the product-moment coefficient has the highly satisfactory value of +0.91 ± .02, but for the women it is exactly 0. The so-called feminine trait of non-predictability seems to be revealed in abundance here. But when one examines the scatter diagram for the women, he finds that one single combination, the white-yellow, stands very much alone. The scatter diagram presents other wise a *negative* correlation. Eliminating the four white-yellow combinations, there is a coefficient of −0.44 ± .09 between the means of the values of the components and the values of the combinations. This indicates that there *is* a law governing affective values, even for the women, but that it is more complex than our first guess.

It will be necessary, in analyzing the law, to determine first of all just how much each one of the components, the more pleasing one and the less pleasing one, contributes to the total effect. In order to do this, we have recourse to the technique of partial correlations. Having found the raw correlation between the more pleasant component (P) and the total combination (C) and also between the more unpleasant member (U) and the total, and finally between the P and U members themselves, the partial coefficients between either P or U and the total combination can be found. This procedure seems permissible, since we are dealing with real values, and since the scatter diagrams approach linearity in form. The coefficients of correlation are given in Table I. For the men they are +0.89 and +0.91 respectively.

[8] J. P. Guilford, The method of paired comparisons as a psychometric method, *Psychol. Rev.*, 35, 1928, 494-506.

[9] J. P. Guilford, Some empirical tests of the method of paired comparisons, *J. Gen. Psychol.*, 5, 1931, 64-76.

TABLE I

COEFFICIENTS OF CORRELATION, REGRESSION COEFFICIENTS, AND STANDARD ERRORS OF ESTIMATE, USING LINEAR EQUATIONS

(P = the more pleasant component; U = the less pleasant component; C = the total combination; p = the predicted values from the regression equation of the first degree; a = the actual or observed values of the combinations; W = the weights or regression coefficients; K = the multiple coefficient of alienation.)

	Raw coefficients		Partial coefficients							
	r_{PC}	r_{UC}	$r_{PC.U}$	$r_{UC.P}$	R_{pa}	S	W_P	W_U	K^2	r_{pa}
Men	.76	.78	.89	.91	.96	.092	.75	.56	.08	.92
Women	.41	−.26	.69	−.51	.72	.208	.86	−.48	.48	.75

This might mean that the P and U components carry equal weight in the combined effect. It will be shown later that this is not quite true. For the women, the partial coefficients are +0.69 and −0.51. This means that the more pleasant the more agreeable color is when alone, the greater will be the agreeableness of of the combination in which it appears, and the more unpleasant the less agreeable color is when alone, the greater is the affective value of the combination. This is a rather startling result, revealing what seems to be a significant sex difference.

The relative weight of each component in the pair is more properly evaluated by finding the regression coefficients. For the men, the P component outweighs the U about 7 to 5. For the women the P component outweighs the U component about 9 to 5, and the latter is a negative value, although relatively small. How are these sex differences to be explained? Perhaps the women are more affected by the total configuration whereas the men tend to analyze the pairs of colors and to balance two independent affective values against two others. It may be an inherent sex difference, or it may be that the women are more sensitive to color combinations as such, from their experiences in choosing colored clothing. Or it may be that women, as usual, took the instructions more seriously and welded the two colors more completely into units than did the men. But this does not tell us why the more unpleasant component in a pair should tend to raise the pleasantness of the combination for the women when it is low (more U), and lower the value of the combination when it is high.

If we wish to approximate the percentage of the total effect that is determined by the total configuration and how much is determined by the individual members as such, we may assume that these two factors are measured by R^2 (R being the multiple coefficient) and K^2 (K being the coefficient of alienation) respectively. R^2 should measure the effectiveness of the two components in determining the total affective value, and K^2 should measure the Gestalt effect itself. For the men the Gestalt effect is only 8% of the whole determination, whereas for the women it is as much as 48%. It must be said that these percentages hold only for these particular predictions from the regression equations which were used

with these data. Other predictive laws might reduce the Gestalt effect to zero. If this is true, K^2 does not measure the alleged Gestalt effect but rather our ignorance of the true law of combination.[10]

Up to this point an equation of the first degree has been assumed. Correlation methods alone do not tell us what the functional relationship is between any two or more variables; they simply tell us how closely our data fit the functions that have been assumed. The regressions of C upon P and upon U are not found to be exactly linear when inspected more closely. The plotted points are rather widely scattered, but the regressions of C on P and U seem to make C a function of some power of P and some root of U. With no theory for a guide, it is largely a matter of cut and fit to discover what the best functions are. One cannot use logarithmic functions of P and U conveniently in this problem, because no absolute zero point has been established for the affective values in question.

As a first guess, the writer used the square root of U and the second power of P. Again the technique of partial correlation was used to test the fit of the data

TABLE II

COEFFICIENTS OF CORRELATION, REGRESSION COEFFICIENTS AND STANDARD ERRORS OF ESTIMATE, USING SECOND POWERS OF P AND ROOTS OF U

	Raw coefficients		Partial coefficients							
	r_{CP^2}	$r_{CU^{\frac{1}{2}}}$	$r_{CP^2 \cdot U^{\frac{1}{2}}}$	$r_{CU^{\frac{1}{2}} \cdot P^2}$	R_{pa}	S	W_{P^2}	$W_{U^{\frac{1}{2}}}$	K^2	r_{pa}
Men	.85	.74	.92	.87	.97	.070	.54	.46	.06	.95
Women	.45	−.39	.74	−.74	.80	.168	.44	−1.02	.36	.76

to the new assumed functions. The scale values were increased by one unit in order to eliminate all negative values so that square roots might be computed for all values of U. The coefficients of correlation, to be seen in Table II, are noticeably higher for both the men and the women. The multiple coefficients, uncorrected for the number of variables, are now 0.97 and 0.80 as compared with 0.95 and 0.75 for the corresponding linear functions. There is still something to be desired by way of prediction, especially in the case of the women. Further analysis of the laws of combination is required.

A very systematic method of analysis is to be found in a recent excellent work of Ezekiel.[11] By a series of successive approximations one can determine the probable nature of the functional relationship between a dependent variable and each one of several independent variables. Thus, one can determine just how

[10]It seems to the writer that this remark may have a wider application to all so-called cases of emergent properties. Such emergent properties might be predicted if all the variables are taken into account, and if their laws of combination are fully approximated. It may be, of course, that among the variables we must include the combining-properties of the several components.

[11]M. Ezekiel, *Methods of Correlation Analysis*, 1930. This book has convinced the writer that in the teaching of statistical methods in psychology we have put the cart before the horse. We have placed undue emphasis upon finding coefficients of correlation. The important thing should be to determine what type of functions we are dealing with and to use the coefficient of correlation as an index to show how well our data fit those functions.

C varies with P, when U is held constant and how it varies with U when P is held constant. The writer has tried to determine the *net* regressions of C on P and on U by this method, and some rather interesting results were obtained. For both men and women, the net regression of C upon U is definitely parabolic. The net regression of C upon P is not nearly so clear, but it is almost linear in form. In some other sets of data which the writer has just secured from colored paper stimuli, and from Geissler's data, similar net regressions are found, except that the relation of C to P tends toward a cubic function in some cases. It is very significant to note that these types of regressions hold equally well for both men and women, and for data obtained from other types of color stimuli.

TABLE III

REGRESSION COEFFICIENTS, MULTIPLE INDICES OF CORRELATION, INDICES OF DETERMINATION, AND STANDARD ERRORS OF ESTIMATE FOR VARIOUS ASSUMED FUNCTIONS

	Regression coefficients							
	P	U	P^2	U^2	PU	$\overline{P}$	$\overline{P^2}$	$\overline{S}$
Men								
	.623	.426	.107	.274	−.336	.937	.878	.114
	.641	.215	.022	.128		.926	.857	.122
	.587	.449	.041			.935	.874	.117
	.672	.451				.937	.878	.113
	.688	.217		.127		.930	.865	.118
	.737		−.027	.273		.935	.874	.117
			.316	.265		.932	.869	.117
		−.065		−.040	.498	.780	.608	.204
Women								
	1.304	−.346	−.054	−.097	.126	.000	.000	.285
	.681	−.187	−.039	−.087		.500	.250	.244
	.755	−.419	−.047			.575	.331	.230
	.733	−.453				.625	.391	.220
	.656	−.193		−.096		.550	.303	.236
	.839		−.045	−.367		.660	.436	.212
			−.061	−.159		.100	.010	.281

The writer next proceeded to adjust the 40 observations to equations involving five independent variables, P^2, P, U^2, U, and PU. The fifth and last variable was added to take care of the possibility that C might be a *joint* function of P and U.[12] That is, the total result of combining affects may be a product rather than a sum. As a matter of fact, this variable (PU) helped not at all, and in several respects led to absurd results. Various predictive functions were assumed, involving two or more of the five variables. The results are given in Table III, which should be interpreted as follows: In line 1 of the table, the predictions were based upon variables P, U, P^2, U^2, and PU. The regression coefficients for each of these variables are 0.623, 0.426, 0.107, 0.274, and −0.336 respectively. The size of the regression coefficient in each case indicates the average amount of change in C for

[12] *Ibid.*, Ch. 20 and 21.

a unit change in the variable to which it belongs, and the sign indicates the direction of the change. $\overline{P}$ is the 'index of multiple correlation,'[13] which has the same meaning as R, but it has been corrected for the number of variables involved. P is always less than R, and is obtained by the use of the formula:

$$\overline{P}^2_{1.234} = 1 - (1 - R^2_{1.234})\,(n - 1)/(n - m)$$

in which n is the number of observations and m is the number of variables. $\overline{P}^2$ is the adjusted 'index of determination' which measures the percentage of variance in C that may be attributed to the variance in the variables involved. $\overline{S}$ is the standard error of estimate, also corrected for the number of variables, and merely shows in another way the variability of the predictions from the observed values. So, continuing to read line 1 of Table III, one would say that the probable true correlation between the predictions and the observed values is 0.937. About 88% of the variance in C may be attributed to the variance in P and U, according to the function assumed. The standard error of estimate indicates that two thirds of the predictions will not deviate from the observed values by more than 0.114 units.

Two or three things about the regression coefficients are noteworthy. We still find the sex differences with regard to the rôle of the U-component. The weights of U and U^2 are invariably negative for the women and are predominantly positive for the men. This might be accounted for on the basis of the five single colors that were used. The white light was the least pleasing for the women as well as for the men, but the former seemed to value combinations of colors with white very highly. But yellow, which was next to the lowest single color for women also behaved more or less like white in combination with others, except when combined with red. Women, it seems, are more inclined to follow the cannons of good taste in art, to avoid the combination of two saturated colors and to prefer the combination of a saturated color with an unsaturated one. It may be that a distinction between the more saturated and the less saturated components will have to be made in the complete law of combination.

An early study of Cohn's[14] tends to show that combinations are more pleasing the farther apart the two colors are in the color circle and also, to some extent, the farther apart they are in brightness. He did not mention specifically the factor of difference in chroma, probably because he chose the most saturated colors possible and hence they were fairly equal in that respect. It may be that these three differences between the two components, the differences in hue, brightness, and in saturation, would account for the remaining 12% of determination in the case of the men and the remaining 60% in the case of the women. It was not easy to take account of these three factors in the data used in this study, and the number of different single colors was probably too limited to give significant results. Eliminating the white light, there would remain only six differences in hue to work with. The major interest in this study has been to find a law of summation of affects, which, from the standpoint of theoretical psychology, is the more important problem. It would be highly interesting, and also very

[13] *Ibid.*, 120 ff.

[14] J. Cohn, Experimentelle Untersuchungen über die Gefühlsbetonung der Farben, Helligkeiten, und ihrer Combinationen, *Phil. Stud.*, 10, 1894, 562-602.

useful from a more practical standpoint to know whether both sets of factors, the values of the components and their relations in the color system, would account for 100% of the results of combination. This is the problem that the writer is now trying to solve with new data.

Another noteworthy fact is that for both sexes the P-component invariably outweighs the U-component in the combination. Most of the single colors, if not all of them, are on the pleasant side of the P-U continuum. The P-components are therefore probably the 'stronger' stimuli so far as feeling is concerned, because they are farther from the indifference point. It may be that stimuli on either side of this point behave differently in combination with others. It may be that a distinction should be made between the stronger and the weaker components. It was impossible to do so in this study, for the indifference point was not determined. The lopsided effect of the two components, P and U, lends some weight to Wundt's second principle of the summation of affect.[15] This principle states that in any combination there is a dominating '*Partialgefühl*' which gives the basic character to the whole. A distinction between this dominant *Partialgefühl* and the other component might help in deriving the true quantitative law.

It will be noted that the multiple correlations in Table III are on the whole smaller than those in Tables I and II. This is due in part to the correction for the number of variables in Table III, and possibly in part to the fact that class intervals and scatter diagrams were used to find the coefficients in Tables I and II. These factors may also be responsible for the differences in the regression coefficients for the linear functions in Tables I and III.

As a matter of fact, the linear functions proved to be among the best for both men and women. Only one function is as good or better in either case. For the men, it is as good as the function involving all five variables. The best one for the women is that involving P, P^2, and U^2. From the successive approximations which were mentioned before, the writer had been led to expect that those functions including P, U, and U^2 would give the best results. Such was not the case. It is quite probable that this is due to the peculiar distribution of the observed values. Had there been more than 40 observations, or more than ten combinations to work with, the relative importance of the various assumed functions might have been different. In view of the fact that the inclusion of P^2 and U^2 does tend to improve the matter noticeably for the women, it would seem that a linear function is not the closest approximation. The fact that most of the multiple coefficients of correlation for the men are about the same, may mean that the limit of prediction has been reached for any law, due to errors of measurement in the data, or to a real Gestalt effect. No coefficients of reliability of the measurements have been computed, so corrections for attenuation cannot be found. For practical purposes, however, several predictive formulae seem to serve equally well. An empirical formula that fits almost perfectly, on the other hand, may not give us a true picture of the law of summation of affect.

There is some indication as to the real nature of affective summation in the peculiar rôle of the product PU. In the case of the men, the inclusion of this variable had little or no effect when all four other variables are included in the formula. For the women, however, the probable true correlation when PU is

[15] *Op. cit.*, 345.

included is zero. Attempts were made to combine PU with two or three other variables, notably with P^2 and U^2. It was thought that C might possibly be a function of $(P + U)^2$ or of $(P - U)^2$. But all such attempts led to impossible results. The multiple correlations were all greater than plus one and $\overline{S}$ was a negative value. Barring any peculiar mathematical 'twists' of which the writer is unaware, it would seem to mean that the law of summation is an additive one and not a product. Further evidence for this fact, aside from the effect of the variable PU, is the use of only P^2 and U^2. In this case, a function of the type $C = f(P + U)(P - U)$ would have a chance to appear a possibility[16]. But in the case of the women, the predicted values correlate only 0.100 with the observed values, and for the men the correlation is somewhat less than the best.

A case might be made for the configurationists and for Wundt and his emergent *Totalgefühl*. Certainly, in the case of the women, predictions are far from perfect, even with the best quadratic equation. Not more than 44% of the variance in C is apparently determined by P and U, granting that no better predictive laws are possible. Even for the men, for whom we seem to have reached the limit of prediction, at least with equations involving second powers of P and U, the percentage of determination is not more than 0.88. We must, of course, allow for chance errors of measurement. If these errors have been accounted for, and if the best law of combination has been found, we can say that $\overline{P}^2$ does measure the extent to which the whole is determined by its parts,[17] and that $(1 - \overline{P}^2)$, or $\overline{K}^2$, the 'index of non-determination', is the extent of the Gestalt effect.

The crux of the configurational question probably lies on the side of perception, unless we may regard the affective reaction as one phase of perception in itself. To what extent do the subjects amalgamate the two colors into a unitary pattern? Only the most extreme behaviorist could overlook this important question. The technique suggested in this paper for the treatment of feelings may also find an application to problems of organization in perception. Indeed, it may be feasible to measure the extent of emergent properties in other departments of psychology, learning, thinking, and even in the field of social behavior concerning which there is much discussion over the contribution of individual behavior to the behavior of a social group.

Within the domain of feeling itself there are many similar problems. Is there a general law of summation of feeling applicable to all sense modalities or even to combinations of stimuli from disparate modalities? Are there different laws of combination for different types of combinations, fusions, colligations, and successions? In the field of aesthetics in general, are there laws of summation of feelings for combinations of forms and colors? These are a few of the questions that suggest themselves. Some of them have been touched upon, as in the studies of Keith,[18] Johnston,[19] and Yokoyama.[20] But nowhere has there been

[16]Ezekiel, *op. cit.*, 277.

[17]*Ibid.*, 120.

[18]J. A. H. Keith, The mutual influence of feelings, *Harvard Psychol. Stud.*, 2, 1906, 141-157.

[19]C. H. Johnston, The combination of feelings, *ibid.*, 159-191.

[20]M. Yokoyama, Affective tendency as conditioned by color and form, this JOURNAL, 32, 1921, 81-107.

any attempt, so far as the writer knows, to state the relationships involved, in quantitative terms. The experiments which have just been mentioned, and other studies of feeling, have discovered some of the qualitative aspects of the problem and have in the main justified the use of the method of paired comparisons for the evaluation of feelings. Such a program as is proposed here, means an approach to a thoroughgoing and scientific system of *psychodynamics*. There are no good reasons why such a program should not be pursued, unless our mathematical tools break down in the attempt.

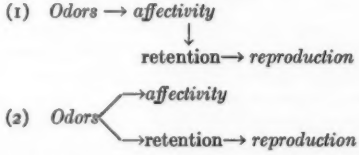
Summary

It is proposed in this study to find a quantitative law by which the affective value of a color combination may be predicted from the known values of its two components, the more pleasing one, P, and the less pleasing one, U. Various mathematical functions, linear and non-linear, are assumed and measurements adjusted to them. Highly valid predictions are possible for the men, but not for the women. There are some indications that the summation of feeling is an additive one and not a product, and that the combined value is not an algebraic sum but a weighted mean of P and U or of some simple functions of P and U. It is suggested that there is a limit to the prediction of an 'affective whole' from its 'components,' and that the emergent, or 'Gestalt-effect' may be measured by the index of non-determination, K^2. It is suggested that this technique be applied to other fields than that of feeling, and that a program of research be initiated leading to a system of psychodynamics.

AFFECTIVE VALUE *VS.* NATURE OF ODORS IN RELATION TO REPRODUCTION

By JEROME D. FRANK, Harvard University

In the report of a previous experiment,[1] Ludvigh and the author concluded, in substance, that pleasant odors presented immediately after learning have a facilitative effect on reproduction, and unpleasant odors an inhibitory one; and that the degree of facilitative or inhibitory effect is proportional to the degree of pleasantness or unpleasantness of the odors as determined by both absolute and rank-order judgments. The experiment involved three variables under direct observation: the test odors as stimuli, the affective value of the odors, and the degree of reproduction of previously learned material. Only two independent hypotheses based on these variables will explain the results obtained. They are (1) that the affective value of the odors is the primary determinant of the degree of reproduction, or (2) that the odors themselves determine both the affectivity and the degree of reproduction, there being no direct causal relation between the reproduction and the affectivity. These hypotheses may be expressed diagrammatically (the observed variables are in italics):



It might be well briefly to consider the claims of a third hypothesis; namely, that the degree of learning, though not influenced by the absolute affectivity of the odors, depends on their relative affective value, *i.e.* their affective value as determined by rank-order judgments. Stated more concretely, this hypothesis maintains that if an odor-group A is judged more pleasant than an odor-group B, then, irrespective of their absolute affective value, group A affects reproduction more favorably than group B. Since the relative affective value of the odors was not, however, actually obtained until the experiment was completed, it remained a mere potentiality during the experiment, and as such could not have influenced the reproduction. All that actually existed were the odors in which the relative affective value was latent; hence this hypothesis reduces to the second one.

*Accepted for publication August 7, 1930.

[1] J. D. Frank and E. J. Ludvigh, The retroactive effect of pleasant and unpleasant odors on learning, this JOURNAL, 43, 1931, 102-108. The phrase "retroactive effect," though in fairly general use, is both inaccurate and misleading, implying, as it does, that an effect can precede its cause. For this reason it has been thought best to shun it altogether in the present paper.

Since in the previous experiment the odors were varied concomitantly with their absolute affectivity, the high correlation obtained between affectivity and reproduction is accounted for on the basis of either hypothesis. A method of distinguishing between them would be to vary the absolute affective value of a single group of odors by means of the principle of affective equilibrium,[2] in all other respects following the same procedure as in the previous experiment. If the degree of reproduction were to vary with the affectivity thus produced, we could assume the correctness of the first hypothesis; if its variations were to show no relation to the variations of the affectivity, the second hypothesis could be considered the correct one. The present experiment is a preliminary investigation of this problem.

As in the previous experiment, 18 odors were used, 6 normally pleasant, 6 normally unpleasant, and 6 normally indifferent. The same four lists, each consisting of 10 pairs of nonsense syllables, were also used; and, as before, they were presented to each *S* in a different order so as to nullify the effect of a particularly easy or difficult list.

The experiment involved four *S*s, all students in Harvard University. They will be referred to throughout by their initials. With *Lo* and *Cu* the procedure was as follows: First, the 6 normally indifferent and 6 normally unpleasant odors were presented in 50 pairs over a period of one hour, by the method of paired comparisons. A rest of not less than 5 min. was allowed after every 10 pairs to guard against sensory adaptation. In the directions, the *S*s were asked to state as each odor was presented, whether they considered it 'very pleasant,' 'pleasant,' 'indifferent,' 'unpleasant,' or 'very unpleasant.' These judgments were scored on the usual $+2$ to -2 scale. At the close of the presentation, the scores for each odor were averaged, and the 6 odors which had the highest average score (*i.e.* were the most pleasant) were then used as the test group throughout the experiment. Though the constitution of this group varied slightly for the different *S*s, for each *S* it was kept constant.

After a 2-day interval, the *S*s were given 10 repetitions of one list of nonsense syllables by means of the Marx memory apparatus.[3] They were then stimulated with the 6 test odors alone and their absolute judgments recorded as above. Thereupon, the first syllables of each pair of the preceding nonsense syllables were presented in random order and the *S*s were asked to give the second. The responses were scored on the basis of three points for a completely correct response, one or two points for partially correct responses, and no credit for wrong responses. Throughout this part of the experiment the conditions were kept rigidly constant. The interval between the presentation of the last pair of syllables and the first pair of odors was 15 sec., the odors were presented in 15 pairs over a period of time which was always within 15 sec. of 8 min., and the total interval between the presentation of the syllables and the recall was kept fixed at 10 min.

After a week the 6 test odors were again presented with the 6 unpleasant ones, and two days later the learning part of the experiment was repeated as above, but with a different set of nonsense syllables.

---

[2] J. G. Beebe-Center, The law of affective equilibrium, this JOURNAL, 41, 1929, 54-69.

[3] For further details see the paper of Ludvigh and the author, *op. cit.*

At least two weeks were then allowed to elapse. After this the test odors were presented to the *Ss* for affective judgment, but this time they were combined with the 6 normally pleasant ones. The absolute judgments were recorded as before. Due to the presence of the normally pleasant odors the judgments on the test odors were shifted toward unpleasantness in accordance with the principle of affective equilibrium. The extent of this shift was determined by comparing the average affective judgments on the odors in the test group, after they had been presented with the normally pleasant odors, with the judgments on the test-group odors obtained previously as a result of presentation with the normally unpleasant odors. If this shift was found not to be pronounced at the first attempt, this part of the experiment was repeated at intervals of from two days to a week until either the test odors were actually judged unpleasant, or further repetitions failed to increase the shift.

Two days after the last presentation the *Ss* were given the learning test, just as before. After an interval of a week, the test odors were again presented with the normally pleasant ones, and two days later the learning test was repeated.

With *We* and *Tw* the procedure was exactly the same, except that the 6 normally indifferent odors were first presented with the 6 normally pleasant ones, then with 6 normally unpleasant. The test group for these *Ss* was therefore composed of the 6 odors, in the combined groups of normally pleasant and normally indifferent odors, which were judged most unpleasant after the first presentation. Thus with *Lo* and *Cu* the absolute judgments on the test odors were shifted from pleasant toward unpleasant; with *We* and *Tw* from unpleasant toward pleasant.

For those desiring to repeat this experiment, it may be pointed out that the elimination of the effect of the learning curve on the results would be most desirable. This could be easily accomplished by repeated memorizing of nonsense syllables before the start of the experiment. Due to lack of time this precaution unfortunately could not be taken in this experiment. It may also be mentioned that apparently the less previous experience with odors the *S* has had, the more easily the shift may be produced.[4] Finally, it was noticed that the longer the time which intervenes before the attempt is made to shift the judgments on the test odors, the more decisive is the shift which can be produced. Hence if a month or even longer were permitted to elapse, the results would be proportionately more conclusive.

Table I presents all the data from the four parts of the experiment involving learning. The numbers 1, 2, 3, and 4, merely represent the temporal order of the tests. In the columns headed "recall score" are given the total scores made by each *S* on his recall of the nonsense syllable list in the trial indicated. The "affective score" is the algebraic sum of the scores corresponding to the absolute judgments given on the test group of odors in the different trials. In each trial the recall score was obtained immediately after the affective score, as explained above. The attempt to shift the absolute affective value of the test odors was made for each *S* between the second and third trials.

[4]That this is the case is suspected for two reasons. In the first place it may be directly deduced from the law of affective equilibrium. Secondly, it was discovered that *Lo*, whose judgments were the most difficult to shift, had done a great deal of work in organic chemistry, which involved the identification of various substances by their odors.

In Table II the data have been arranged so as to reveal the effect of the four shifts produced by affective equilibrium. The affective scores in the first and second test periods have been averaged for each *S*, as have the affective scores in the third and fourth periods. The same averages have been computed for the corresponding recall scores. The figures indicating difference in average affective scores are obtained by subtracting the average score of trials 1 and 2 from that of trials 3 and 4. The differences in average recall scores are obtained in the same manner. As explained above, the judgments of *We* and *Tw* were shifted toward increased affectivity, those of *Lo* and *Cu* toward decreased affectivity.

TABLE I

SHOWING FOR EVERY *S* THE AFFECTIVE AND RECALL SCORES AT EVERY ONE OF THE TEST PERIODS

	1		2		3		4	
S	Aff. score	Recall score	Aff. score	Recall score	Aff. score	Recall score	Aff. score	Recall score
We	−10	9	−24	3	+ 6	6	+10	4
Tw	− 9	16	−17	30	− 2	52	− 4	28
Lo	+29	15	+25	15	+17	26	+19	18
Cu	+ 5	13	+ 1	24	−18	24	−15	29

TABLE II

SHOWING FOR EVERY *S* THE AVERAGE AFFECTIVE AND RECALL SCORES OF THE FIRST AND SECOND AND OF THE THIRD AND FOURTH TEST PERIODS, AND THE DIFFERENCES BETWEEN THE AVERAGES

	1 and 2		3 and 4		Difference	
S	Av. aff. score	Av. recall score	Av. aff. score	Av. recall score	in aff. scores	in recall scores
We	−17	6	+ 8	5	+25	− 1
Tw	−13	46	− 3	53	+10	+ 7
Lo	+27	15	+18	22	− 9	+ 7
Cu	+ 3	13.5	−16.5	26.5	−19.5	+13

It will be seen that the shifts in learning of 3 of the 4 *S*s are directly opposed to the shifts in affective judgment. In the one case in which the learning and affectivity shift in the same direction there is no reason to assume that the increase in learning is produced by the corresponding increase in affectivity, since it is fully accounted for by the influence of the learning curve. Hence in no case is there any evidence that the shift in affectivity produces a corresponding shift in learning.

A more detailed analysis of the results is possible. If a shift in affective value, under the conditions of this experiment, determines a change in learning, then it should do so not only between those trials in which the affective scores were intentionally shifted, but also between those trials in which the shift in affective value was not purposely produced. Although, as pointed out above, the intentional affective shifts show no evidence of having produced corresponding changes in the learning, it is yet possible that an examination of the affects of the accidental shifts might reveal enough evidence of the opposite sort to make a negative

conclusion unwarranted. In order to investigate this possibility, Table III was compiled. The figures indicating the difference in affective score between trials 1 and 2 were obtained by subtracting the affective score made by each *S* in trial 1 from that made in trial 2. The same procedure was followed in determining the other differences in affective scores. The figures indicating differences in recall score were obtained in the same manner.

TABLE III

SHOWING FOR EVERY *S* THE DIFFERENCES IN THE AFFECTIVE AND RECALL SCORES BETWEEN THE SUCCESSIVE TEST PERIODS

S	Diff. between 1 and 2		Diff. between 2 and 3		Diff. between 3 and 4	
	Aff.	Recall	Aff.	Recall	Aff.	Recall
We	−4	− 6	+30	+ 3	+4	−2
Tw	−8	+14	+15	− 5	−2	+3
Lo	−4	0	− 8	+11	+2	−8
Cu	−4	+11	−17	0	−3	+5

This table may be summarized as follows: four *increases* in affective score correspond to (a) three decreases in recall score, and (b) one increase in recall score; eight *decreases* in affective score correspond to (a) five increases in recall score; (b) one decrease in recall score, and (c) two unchanged recall scores. Thus in only two out of the twelve instances is the shift in learning in the same direction as the shift in affectivity. Furthermore, one of these two is in the direction of increased learning, and hence might well be due to learning.

A further method of treating the data is to determine the coefficient of correlation between shift in affective score and shift in recall score from one trial of an observer to another. For this purpose the shifts in affective score and shifts in recall score in Table III were arranged in corresponding columns and the Pearson formula applied. It was found that $r = -0.16$, P.E. $= 0.21$. This correlation obviously fails to show any positive relationship between shifts in affectivity and corresponding shifts in recall score.

CONCLUSION

Due to the slender proportions of this experiment, its chief value lies rather in its suggestion of a method of attacking the problem than in the conclusions to be drawn from the results. Since the data all seem to point in one direction, however, we may conclude, tentatively, that, when the odors remain constant, a shift in their affective value as determined by absolute judgments has no corresponding effect on the reproduction of the syllables. It follows that of the two hypotheses presented at the beginning of this paper, the second is presumably the correct one. It would seem that the odors themselves determine the degree of reproduction, and that there is no direct causal relation between the reproduction and the affectivity.

This hypothesis can be further verified since it implies that, if the test odors are varied while the affectivity is kept constant, the correlation between affectivity and response score should again be no more than zero, while the degree of reproduction should vary with the odor groups used. The writer hopes to investigate this aspect of the problem in the near future.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

LXI. The Influence of the Size of an Outline Cube on the Fluctuations of Its Perspective

By M. F. Washburn, H. Mallay, and A. Naylor

The phenomena of what have been called fluctuations of attention need a thorough re-investigation, for the actual quantitative data which we have regarding them are very scanty. They are certainly complex, the resultant of various influences, and we must patiently accumulate more measurements under varied conditions before we can solve the problems they present. It is the hope of the Vassar laboratory to do, in the next few years, something towards this end. The present study represents a beginning. Its problem is to determine the effect of the size of an outline cube on the duration and the variability of the duration of a single perspective interpretation of the cube; that is, the average length of time it is seen by the *O* as resting on the ground or suspended in the air before the opposite interpretation sets in, and the average deviation of these averages.

Method and Procedure

Three cubes were drawn, each on a sheet of carboard, by an expert draughtsman: a large cube 50 cm. a side, a medium-sized cube, 5 cm. a side, and a small cube, 5 mm. a side. In an experiment, one of these figures was fastened to an upright surface in the dark room. A headrest was clamped to the edge of the table, so that the *O*'s eyes were about 60 cm. from the diagram. Light came from a 50-watt ground glass electric bulb about 30 cm. over the *O*'s head. The *O* adjusted the headrest at a comfortable height. She was instructed to fixate the point A (Fig. 1) steadily, and to say 'Up' when the cube appeared to be hanging in the air, and 'Down' when it appeared to be resting on the ground. (There was no lettering on the cubes, and the fixation point was not marked.) In such experiments the less *O* has to do the less, in all probability, the fluctuation is interfered with. During the experiment a metronome ticked at the rate of 120 per minute, and with each tick *E* made a short pencil mark on paper. When *O* said, 'Up,' *E* began to make vertical marks; when *O* said, 'Down,' *E* changed to horizontal marks; thus the marking indicated half-seconds with an accuracy sufficient for a problem of this kind, in which very slight time-differences would probably have no significance. The total time of an experiment was 3 min.; at the end *O* rested her eyes. Three such experiments were made at a sitting, and there were 6 sittings, each on a different day, for each *O*. In one experiment in a sitting the large cube was used first; in another the medium cube, and in another the small

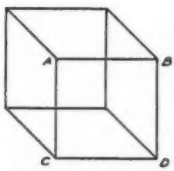


Fig. 1. Cube with Reversible Perspective

*Accepted for publication May 15, 1931.

cube: to equalize the fatigue factor the order was as follows: Day 1, S, M, L; Day 2, L, M, S; Day 3, M, S, L; Day 4, S, L, M; Day 5, L, S, M; Day 6, M, L, S. There were 12 *Os*, all young women college students.

Results

(*A*) *Average duration of down and up phases for each O and each cube.* The following are the average durations in seconds throughout the 3-min. interval.

Down phase. Observer *A.N.*, small cube, 2.17; medium cube, 2.22; large cube, 2.32: *V.W.*, small cube, 2.65; medium cube, 3.59; large cube, 4.61: *E.B.*, small cube, 4.64; medium cube, 5.65; large cube, 9.30: *R.M.*, small cube, 3.09; medium cube, 5.38; large cube, 24.78: *A.W.*, small cube, 5.14; medium cube, 8.70; large cube, 23.17: *I.L.*, small cube, 2.06; medium cube, 2.46; large cube, 3.01: *H.M.*, small cube, 4.19; medium cube, 4.47; large cube, 4.19: *H.D.*, small cube, 1.71; medium cube, 1.87; large cube, 1.53: *E.S.*, small cube, 3.03; medium cube, 3.70; large cube, 3.63: *I.C.*, small cube, 5.55; medium cube, 6.99; large cube, 11.23: *A.H.*, small cube, 2.26; medium cube, 2.89; large cube, 3.26: *M.C.*, small cube, 3.11; medium cube, 4.43; large cube, 4.74.

Up phase. Observer *A.N.*, small cube, 2.13; medium cube, 2.30; large cube, 2.11: *V.W.*, small cube, 2.15; medium cube, 2.33; large cube, 2.39: *E.B.*, small cube, 3.26; medium cube, 2.73; large cube, 5.03: *R.M.*, small cube, 3.75; medium cube, 7.14; large cube, 12.34: *A.W.*, small cube, 4.12; medium cube, 6.01; large cube, 14.35: *I.L.*, small cube, 2.53; medium cube, 2.70; large cube, 7.35: *H.M.*, small cube, 4.27; medium cube, 3.84; large cube, 4.71: *H.D.*, small cube, 1.85; medium cube, 2.08; large cube, 2.05: *E.S.*, small cube, 2.69; medium cube, 3.13; large cube, 3.39: *I.C.*, small cube, 4.51; medium cube, 3.49; large cube, 5.17: *A.H.*, small cube, 2.47; medium cube, 2.92; large cube, 2.77; *M.C.*, small cube, 2.51; medium cube, 4.01; large cube, 3.90.

It will be seen that in 21 out of these 24 cases the large cube had a fluctuation-rate slower than that of the small cube (the exceptions being *H.M.*, down phase, *H.D.*, down phase, and *A.N.*, up phase). In 17 cases out of the 24 the large cube had the slowest rate of the three (the exceptions being, besides those mentioned above, *H.D.*, up phase; *E.S.*, down phase; *A.H.*, up phase, and *M.C.*, up phase). In 19 cases out of the 24 the small cube had the fastest fluctuation rate of the three (the exceptions being *H.M.*, down; *H.D.*, down; *A.N.*, up; *E.B.*, up; *I.C.*, up).

(*1*) *There seems to be a tendency for the small cube to fluctuate faster than the large cube.*

(*2*) *This tendency is much more marked in some individuals than in others.* The following figures show how strong it is in each of our 12 *Os*: after each *O*'s initials there follows the percentage which the difference in the average duration of phases (both up and down) for the large cube and that for the small cube forms of the average duration of phases in the small cube, for this particular *O*: (1) *R.M.*, 160%; (2) *A.W.*, 138%; (3) *E.B.*, 63%; (4) *I.L.*, 60%; (5.5) *I.C.*, 51%; (5.5) *H.M.*, 51%; (7) *M.C.*, 40%; (8) *V.W.*, 37%; (9) *A.H.*, 34.7%; (10) *E.S.*, 19%; (11) *A.N.*, 2.9%; (12) *H.D.*, 0.005%.

(*3*) *Those Os having the greatest tendency to slow the alternations of the large cube are in general those for whom all fluctuations are slowest.* The following figures state for each *O* the average duration of all fluctuations, for all three cubes: (1)

A.W., 10.25; (2) *R.M.*, 9.41; (3) *I.C.*, 6.16; (4) *E.B.*, 5.10; (5) *H.M.*, 4.27; (6) *M.C.*, 3.78; (7) *E.S.*, 3.26; (8) *I.L.*, 3.02; (9) *V.W.*, 2.95; (10) *A.H.*, 2.76; (11) *A.N.*, 2.21; (12) *H.D.*, 1.85. The r between this order and that of the difference between large cube and small cube speed of fluctuation is $+0.87 \pm 0.05$.

This correlation is of course partly if not largely due to the fact that while under the conditions of the experiment fluctuations can be lengthened up to the total duration of an experiment, 3 min., in which case there would be no fluctuation, they cannot be shortened much below a 2-sec. limit; thus the larger differences between large-cube and small-cube rates will be due rather to extra lengthening of the large-cube phases than to extra shortening of the small-cube phases; hence they will correlate with long average (small *plus* large) fluctuations. That this is not the only factor, however, is indicated by the fact that some tendency exists for those *Os* who have the longer *small*-cube rates to show the greatest differences between length of large- and small-cube rates. The order of *Os* in length of small-cube fluctuations is: (1) *I.C.*, 5.03; (2) *A.W.*, 4.63; (3) *H.M.*, 4.23; (4) *E.B.*, 3.95; (5) *R.M.*, 3.42; (6) *E.S.*, 2.86; (7) *M.C.*, 2.81; (8) *V.W.*, 2.40; (9) *A.H.*, 2.36; (10) *I.L.*, 2.29; (11) *A.N.*, 2.15; (12) *H.D.*, 1.78. This gives an r with differences between large- and small-cube rates of $+0.67 \pm 0.11$.

(*B*) *Average variability of the duration of the phases during the 3-min. interval: difference between large and small cubes.*

The average duration of phases, whether up or down, was found for each cube and each *O*, and its average deviation was divided by the average to find the percent variability. This was as follows: for *A.N.*, small cube, 51%; medium cube, 66%; large cube, 59%; for *V.W.*, small cube, 50%; medium cube, 50%; large cube, 62%: for *E.B.*, small cube, 55%; medium cube, 55%; large cube, 68%: for *R.M.*, small cube, 52%; medium cube, 66%; large cube, 96%: for *A.W.*, small cube, 45%; medium cube, 52.7%; large cube, 64%: for *I.L.*, small cube, 52%; medium cube, 58%; large cube, 74%: for *H.M.*, small cube, 65%; medium cube, 50%; large cube, 43.8%; for *H.D.*, small cube, 25%; medium cube, 20%; large cube, 28%: for *E.S.*, small cube, 43%; medium cube, 41%; large cube, 50%: for *I.C.*, small cube, 45%; medium cube, 58%; large cube, 55%: for *A.H.*, small cube, 43%; medium cube, 43%; large cube, 44%: for *M.C.*, small cube, 37%; medium cube, 43%; large cube, 47%.

(*1*) *Those Os for whom the large-cube fluctuations are slowest as compared with the small-cube fluctuations tend to be the Os for whom the percent variability of fluctuation-length during the 3-min. interval is most noticeably greater for the large cube.*

The following is the order of the *Os* with regard to the amount by which the percent variability of phase length in the large cube is greater than that in the small cube: (1) *R.M.*, 44; (2) *I.L.*, 22; (3) *E.B.*, 13; (4) *V.W.*, 4; (5.5) *I.C.*, 10; (5.5) *M.C.*, 10; (7) *A.W.*, 9; (8) *A.N.*, 8; (9) *E.S.*, 7; (10) *H.D.*, 3; (11) *A.H.*, 1; (12) *H.M.*, -21. The r of this order with that of the *Os* ranked according to the amount by which the length of the large-cube fluctuations exceeds that of the small-cube fluctuations is $+0.63 \pm 0.12$.

(*C*) *Average variability of fluctuations on different days: difference between large and small cubes.*

(*1*) *Those Os for whom the large-cube fluctuations are slowest compared to the small-cube fluctuations have some tendency to be those whose percent variability in different sittings, i.e. on different days, is most noticeably greater for the large cube.*

The following is the order of the *Os* with regard to the amount by which the percent variability in phase-length on different days is most noticeably greater for the large cube: (1) *R.M.*, 79.5; (2) *E.B.*, 38.25; (3) *I.C.*, 35.5; (4) *M.C.*, 20.8; (5) *A.H.*, 17.5; (6.5) *A.W.*, 16; (6.5) *V.W.*, 16; (8) *H.M.*, 7.2; (9) 4.5; (10) *H.D.*, 1.0; (11) *E.S.*, −1.75; (12) *I.L.*, −5.

The *r* of this order with that of the *Os* ranked according to the amount by which the length of the large-cube fluctuations exceeds that of the small-cube fluctuations is $+0.54 \pm 0.14$.

When we try to discover the principles involved in this rather confusing mass of data, the probability emerges that we are dealing with two types of *Os*. The first type experiences a short-rate and comparatively regular fluctuation of cube-phase (*A.N.*, rank in average total duration of phases, 11; *H.D.*, rank 12; *A.H.*, rank 10). This rate is little affected by the size of the cube (*A.N.*, rank in percent difference between fluctuation rates of large and small cubes, 11; *H.D.*, rank 12; *A.H.*, rank 9). The variability of the duration phases during the 3-min. interval for *Os* of this type is little affected by the size of the cube (*A.N.*, rank in difference between large and small cubes as regards their percent variability during the 3-min. interval, 8; *H.D.*, rank 10; *A.H.*, rank 11). The variability of the duration of phases on different days is for this group little affected by the size of the cube (*A.N.*, rank in difference between small and large cubes as regards their percent variability from day to day, 9; *H.D.*, 10; *A.H.*, 5).

Thus this type of *O* shows a short-rate fluctuation little influenced either in length or in variability by differences in the size of the cube.

The second type of *O* experiences a relatively long and irregular average duration of phase (*A.W.*, rank in average total duration of phases, 1; *R.M.*, rank 2; *I.C.*, rank 3; *E.B.*, rank 4). This rate is markedly affected by the size of the cube (*A.W.*, rank in percent difference between fluctuation rates of large and small cubes, 2; *R.M.*, rank 1; *I.C.*, rank 5.5; *E.B.*, rank 3). The variability of the length of phases during the 3-min. interval is for *Os* of this type more affected by the size of the cubes than for *Os* of the first type (*A.W.*, rank in difference between large and small cubes as regards their percent variability during the 3-min. interval, 7; *R.M.*, rank 1; *I.C.*, rank 5.5; *E.B.*, rank 3). The average of these ranks is 4.12; the average of the corresponding ranks of the first type of *Os* is 9.6; this difference allows 98 chances out of 100 of a true difference. The variability in the length of phases on different days is also more affected by the size of the cube for this type of *Os* than for the first type. The ranks in difference between large and small cubes in percent variability on different days are: *A.W.*, 6.5; *R.M.*, 1; *I.C.*, 3; *E.B.*, 2. The average of these ranks is 3.1; that of the corresponding ranks of *Os* belonging to the first type is 8; the difference gives 94 chances out of 100 of a true difference.

A reasonable interpretation of this difference between two types of *Os* would be that in the second type a factor or factors are superadded to the factor causing the regular short-period fluctuations experienced by the first type. This additional factor does two things: it tends to slow all fluctuations, including those of the small cube, and from time to time it greatly slows the fluctuation of the large cube.

The evidence that it slows the fluctuations of the small cube is clear from the following figures: for the *Os* of the first type, who lacked this additional factor, the average duration of small-cube fluctuation is: *A.N.*, 2.15; *H.D.*, 1.78; *A.H.*, 2.36. For the *Os* of the second type it is: *A.W.*, 4.63; *R.M.*, 3.42; *I.C.*, 5.03; *E.B.*, 3.95. The difference between the averages for these two types is thoroughly reliable.

The evidence that the additional factor, or some additional factor, occasionally greatly lengthens the phase of the large cube is clear from the following figures:

For the *Os* of the first type there was never, on any of the six days of experiment or during any single minute of the three-minute period, a significant difference between the length of small-cube phases and that of large-cube phases.

Among *Os* of the second type, *R.M.*, on the first and second days of experimenting, experienced hardly any fluctuation at all of the large cube; it remained in the down phase. The total amount by which the length of large-cube phases exceeded that of the small-cube phases during the 3-min. interval was on the 1st day 121 sec.; on the 2nd day 125.4 sec. On the 3rd day and 5th days there was no difference between large- and small-cube fluctuations; on the 4th day it rose to 24.4 sec., and on the 6th day amounted to 7.38 sec.

For *A.W.*, also, on the 1st day the large cube fluctuated very slowly indeed throughout the 3-min. period, the total time by which the length of its phases exceeded that of the small-cube phases being 102.4 sec. There continued on the 2nd, 3rd, 4th, and 5th days to be noticeable, though much smaller, difference between large- and small-cube rates: the length of large-cube phases exceeded that of small-cube phases on the 2nd day by 18.15 sec.; on the 3rd day by 17.51 sec.; on the 4th day by 17.72 sec., on the 5th day by 36.06 sec.; on the 6th day it dropped to 5.88 sec.

For *E.B.*, also, the large cube fluctuated more slowly on the first day than on any succeeding days: the difference by which the length of its phases exceeded that of the small-cube phases was 23.17 sec. On the 2nd, 4th, and 6th days it was negligible; on the 3rd and 5th days it was respectively 15.87 sec. and 16.92 sec.

For *I.C.*, during the first minute on the first day the large cube did not fluctuate at all, but remained in the down phase; the total amount by which large-cube fluctuations exceeded small-cube fluctuations was 79.22 sec. On the 2nd, 4th, 5th, and 6th days the difference was negligible; for the third day it was 46.21 sec.

None of the remaining *Os* showed large differences between large-cube and small-cube fluctuation rates.

Is it possible to conjecture the nature of any factor that may have operated, in addition to that producing the regular short-phase fluctuations for all cubes which appeared in *Os* of the first type, to produce in those of the second type a slowing of all fluctuations and more or less often a great lengthening of large-cube fluctuations?

In the earlier days when fluctuations of ambiguous figures were studied, shift of eye-fixation due to fatigue was regarded as an important, if not the sole, cause of such fluctuations. Titchener,[1] in his discussion of illusions of reversible perspective, emphasizes the fact that the point fixated on an outline cube appears

[1] E. B. Titchener, *Experimental Psychology*, 1901, I, pt. 2, 310.

nearer to the observer. In our experiments steady fixation on the point A was required throughout; this would tend to bring the face ABCD nearer the *O*, and thus would favor the down phase of the cube. It is practically certain, of course, that such a fixation could not have been constant through the experiments. Further, it seems plausible that slight lapses in fixation would be more important in the case of the smaller cube than in that of the larger cube.

We know that shift of fixation is not the main cause of cube-fluctuations. One conclusive observation that disproves such a supposition is that if two small cubes be drawn side by side and a fixation point marked between them, they will often be seen in opposite phases at the same time. But may not some *O*s be more influenced than others by the factor of slight lapses in fixation, superadded to whatever is the cause of the regular short-period fluctuations?

If fixation, a factor on which difference in the size of the cubes would be influential, were really the influence operating on the *O*s of the second group but not on those of the first group, it might well appear that the *O*s of the second group were those who tended to see the cubes longer in the down phase, the one favored by the fixation-point prescribed throughout, than in the up phase. The *O*s of the first group, whose fluctuations were not influenced by fixation, would show less dominance of the down phase.

This conjecture is borne out by the facts. The average time in seconds, for small, medium, and large cubes taken together, by which the down phase exceeded the up phase in duration is for *A.N.*, 0.17; for *H.D.*, −0.87; for *A.H.*, 0.25. The corresponding differences for the *O*s of the second group are for *A.W.*, 12.23; for *R.M.*, 10.02; for *I.C.*, 16.60; for *E.B.*, 8.51. These were the four highest differences, and those of the first group of *O*s were ranked tenth, twelfth, and ninth.

Conclusions

At least two factors seem to influence the alternations of interpretation in outline cubes of different sizes. One, which cannot be further identified, and which is the only factor influencing the alternations of phase in 3 out of 12 *O*s, produces a steady, short-wave alternation, unaffected by the differences in the size of the cube used in this experiment, varying but little in the course of a 3-min. sitting, and but little on different experimental days. Its average lengths for each of the 3 *O*s, were 1.85, 2.21, and 2.76 sec. The other factor, either superimposed on the first or working alone, causes longer fluctuations in all cubes; and is markedly influenced by the size of the cube, producing longer fluctuations in the large cube, and occasionally total absence of fluctuation in the large cube. This factor is highly variable in its action both during a 3-min. interval and on different days. The supposition that this factor is due to shifting of fixation receives some support from the fact that the 4 *O*s in whom it is most manifest are those for whom the down phase of the cubes, which is favored by the fixation point prescribed throughout these experiments, most prevails over the up phase; they are thus the *O*s most influenced by the location of the fixation point.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

LXXIX. The Response of Cold Spots under Successive Stimulation

By M. Bullard Drury and Karl M. Dallenbach

To avoid the effects of fatigue and 'tuning,' which are little understood, experimenters in the field of the temperature senses have been extremely cautious with the repetition of their stimulations—especially when working with punctiform stimulus-objects and with single skin-spots. The most careful experimenters have not stimulated the same spot with the same stimulus-temperature during the same experimental hour. This precaution not only prolongs the duration of the experiment, but increases greatly the difficulty of locating and of stimulating the same spot at a later period. The object of the present study is to learn whether this precaution is necessary: whether, in other words, the long delays between successive stimulations of the same spot, which tradition dictates, are necessary for accurate results. We hoped, by using time-intervals of various lengths, to determine the maximum rate of stimulation at which a spot will continue to respond. We have limited ourselves in the present study to the stimulation of cold spots. Since it would be gratuitous to assume that the results obtained for cold spots would hold for warm, we hope at a later date to repeat the work with warm spots.

Method and Procedure

Two small areas, 5 x 5 cm. sq., on the dorsal surface of *O*'s left forearm were selected as the experimental tissues; one area was near the middle of the biceps humeri, and the other near the middle of the dorsal forearm. Both areas were shaved before the experimental work began, so as to avoid the distractions which might result from touching the hairs. During the experiments *O*'s arm was held firmly in place by a plaster of Paris arm-rest, cast especially for him.

Ten cold spots—5 upon each experimental area—were located and marked with India ink; a hand magnifying glass was used in this part of the work. After the spots were marked, a 20-min. interval was allowed to elapse before the main experiments were begun.

Apparatus. The spots were stimulated by means of a temperature aesthesiometer.[1] The stimulus-point was cooled by a stream of water, drawn through the apparatus by means of an hydraulic siphon. The temperature of the water-bath was constant between 4° and 5°C.

*Accepted for publication July 16, 1930.

[1] For the description of this apparatus, see K. M. Dallenbach, Some new apparatus, this Journal, 34, 1923, 92 ff.; also, The temperature spots and end-organs, *ibid.*, 39, 1927, 413 f.

Observers. Three *Os* served in the experiment: Dr. George Kreezer (*K*), instructor in psychology; Mr. C. W. Fox (*F*), assistant; and the junior author (*Dr*).

Instructions. The following instructions were read to *O* at the beginning of the experimental periods: "You will be stimulated with a cold point. Report when you experience cold, and then describe the intensity of your experience in terms of the following 4-point scale: intense, moderate, mild, and weak."

Procedure. Twenty minutes after they were marked, the 10 cold spots were stimulated 10 times each and at 6 different time-intervals—5, 15, and 30 sec., and 1, 3, and 5 min. The longer intervals were used first. Since the initial stimulations of every spot at every interval—60 in all—were preceded by a 20-min. rest, they were treated as a separate group of experiments involving an interval of 20 min. This left 9 stimulations of every spot for every one of the other 6 intervals, *i.e.* 90 at every interval.

The pressure was kept uniform by means of the aesthesiometer, and the rate as follows: *E* counted "one, two, three, four" silently and in rhythm with a silent seconds-pendulum, applying the stimulus-point to a spot on the count of 'one,' removing it on the count of 'two,' holding it off the skin during the counts of 'three' and 'four,' and then applying it again to another spot on the count of 'one,' and so on until every one of the spots had been stimulated. Then, after the predetermined interval, the procedure was repeated until every marked spot had been stimulated 10 times.

With the longer intervals, the 10 spots were stimulated in series. As 10 stimulations took 40 sec. (4 sec. for every spot) the procedure had to be changed for the three shortest intervals (30, 15, and 5 sec.). With the 30-sec. interval, therefore, only 5 spots were stimulated in series; and with the 15- and 5-sec. intervals, each spot in turn was stimulated 10 consecutive times.

A few unmarked spots—some sensitive to cold and some not—were stimulated, between series but in rhythm with the marked spots, so that *O* could not know when a marked spot was being stimulated, and so that he would have a ready means at hand of gauging his intensive judgments.

This study was made in June 1930. The temperature of the room varied between 24-26°C. during the experiments.

Results

The gross results of the study are given in Table I, which shows the number of times the cold spots responded at every interval. Since the procedure in the case of the 20-min. interval was somewhat different, we consider first the other 6 intervals alone, and in their case we find that the spots responded 82-96% of the time for *Dr*, 81-98% for *F*, and 87-100% for *K*. There is, however, no agreement among the *Os* on the most or least favorable rate. The highest frequency of response coincides with the interval of 5 min. for *K*, 3 min. for *Dr*, 1 min. for *F*; the lowest, with 5 sec. for *Dr*, 15 sec. and 1 min. for *K*, 3 min. for *F*.

All the *Os*, however, reported 'cold' more frequently with the 3 longer than with the 3 shorter intervals. As is shown in Table II, where the results of the long and of the short intervals are combined, *Dr* reported 'cold' 253 times with the long, and 229 times with the short intervals; *F*, 244 and 238 times respectively; and *K*, 251 and 239 times respectively.

TABLE I

SHOWING, FOR EVERY INTERVAL, THE NUMBER OF TIMES COLD WAS REPORTED AT THE VARIOUS INTENSITY-LEVELS, AND THE NUMBER OF TIMES COLD WAS NOT REPORTED

		Cold					Not Cold			
							Stimulations			
O	Interval	Intense	Moderate	Mild	Weak	Total	Total	2-4	5-7	8-10
	20 min.*	18	57	15	0	90	0	—	—	—
	5 min.	9	35	40	0	84	6	1	2	3
	3 min.	13	43	30	0	86	4	0	2	2
Dr	1 min.	7	43	33	0	83	7	1	2	4
	30 sec.	4	37	35	0	76	14	5	4	5
	15 sec.	7	31	36	5	79	11	0	4	7
	5 sec.	7	35	32	0	74	16	1	4	11
	20 min.*	7	50	33	0	90	0	—	—	—
	5 min.	3	27	53	0	83	7	2	2	3
F	3 min.	1	20	52	0	73	17	2	8	7
	1 min.	0	43	45	0	88	2	0	2	0
	30 sec.	2	15	57	0	74	16	3	7	6
	15 sec.	0	14	72	0	86	4	1	3	0
	5 sec.	0	21	57	0	78	12	3	5	4
	20 min.*	29	54	6	0	89	1	—	—	—
	5 min.	22	37	31	0	90	0	0	0	0
	3 min.	24	41	18	0	83	7	2	3	2
K	1 min.	9	42	27	0	78	12	2	4	6
	30 sec.	6	45	21	8	80	10	4	1	5
	15 sec.	2	38	26	12	78	12	2	5	5
	5 sec.	7	33	19	22	81	9	1	4	4
	20 min.	54	161	54	0	269	1	—	—	—
	5 min.	34	99	124	0	257	13	3	4	6
	3 min.	38	104	100	0	242	28	4	13	11
Total	1 min.	16	128	105	0	249	21	3	8	10
	30 sec.	11	97	113	8	230	40	12	12	16
	15 sec.	9	83	134	17	243	27	3	12	12
	5 sec.	14	89	108	22	233	37	5	13	19

*The reports at this interval—60 in number—were multiplied, for comparison, by 3/2 so as to bring the number up to 90—the number of stimulations at the other intervals.

TABLE II

SHOWING, FOR INTERVALS OF 1, 3, and 5 MIN. and of 5, 15, and 30 SEC., THE NUMBER OF TIMES COLD WAS REPORTED AT THE VARIOUS INTENSITY-LEVELS, AND THE NUMBER OF TIMES COLD WAS NOT REPORTED

		Cold					Not Cold			
							Stimulations			
O	Intervals	Intense	Moderate	Mild	Weak	Total	Total	2-4	5-7	8-10
	1 min. +	29	131	93	0	253	17	2	6	9
Dr	30 sec. −	18	103	103	5	229	41	6	12	23
	Total	47	234	196	5	482	58	8	18	32
	1 min. +	4	90	150	0	244	26	4	12	10
F	30 sec. −	2	50	186	0	238	32	7	15	10
	Total	6	140	336	0	482	58	11	27	20
	1 min. +	55	120	76	0	251	19	4	7	8
K	30 sec. −	15	116	66	42	239	31	7	10	14
	Total	70	236	142	42	490	50	11	17	22
	1 min. +	88	341	319	0	748	62	10	25	27
Total	30 sec. −	35	269	355	47	706	104	20	37	47
	Total	123	610	674	47	1454	166	30	62	74

TABLE III

SHOWING THE AVERAGE NUMBER OF REPORTS OF COLD AFTER THE LONG AND THE SHORT INTERVALS, AND THE SIGNIFICANCE OF THE DIFFERENCE BETWEEN THE AVERAGES

O	Ave. of 1-, 3-, and 5-min. intervals		Ave. of 5-, 15-, and 30-sec. intervals		D	σ diff.	D/σ diff.
Dr	84.33	S.D. 1.53	76.33	S.D. 2.52	8.00	1.70	4.70
F	81.33	S.D. 7.64	79.33	S.D. 6.11	2.00	5.65	0.35
K	83.66	S.D. 6.03	79.66	S.D. 1.53	4.00	3.59	1.11
All	83.11	S.D. 5.11	78.44	S.D. 3.75	4.67	2.11	2.21

Table III, which gives the averages for the long and the short intervals and the reliabilities of the differences between them (D/σ diff.),[2] shows that the differences are statistically reliable when the results of all the *O*s are considered together, but that they are reliable for only one *O* (*Dr*) when the results of the *O*s are considered separately. The difference between the averages of the long and of the short intervals for all the *O*s is 4.67, and the D/σ diff. is 2.21—98.6 times in 100 it would be in the same direction. *Dr*'s difference is 8, and the D/σ diff. is

[2] Because of the small number of cases involved σ was computed by the formula $\sigma = [\Sigma d^2/(n-1)]^{\frac{1}{2}}$.

4.70—99.9+ times in 100 it would be in the same direction. *F*'s and *K*'s differences are, however, small (2 and 4 respectively) and of doubtful significance (D/σ diff. being 0.35 and 1.11 respectively). The length of the interval between repeated stimulations of cold spots seems to have little effect upon their reports. One temporal interval, within the limits of 5 sec. to 5 min., is about as good for them as another.

The results of the 20-min. interval are given in Table I. Not only is cold reported more often—by *Dr* and *F* at every stimulation, and by *K* at all but one—but the intensities reported are higher. More reports of intense and moderate cold, and fewer of mild and weak, were given at this interval than at any other used.

The effect of interval upon intensity is evident in the other 6 intervals also. In Table II the curve of the intensive reports is skewed for all the *O*s toward the higher levels at the longer intervals, and toward the lower levels at the shorter intervals.

TABLE IV

SHOWING THE NUMBER OF TIMES COLD WAS NOT REPORTED AT EVERY ONE OF THE 10 STIMULATIONS AT THE LONG AND THE SHORT INTERVALS

O	Intervals	Stimulations										Grand
		1	2	3	4	5	6	7	8	9	10	Total
	1 min. +	0	0	2	0	1	2	3	4	2	3	17
Dr	30 sec. −	0	1	1	4	3	3	6	6	8	9	41
	Total	0	1	3	4	4	5	9	10	10	12	58
	1 min. +	0	0	1	3	7	3	2	2	5	3	26
F	30 sec. −	0	1	3	3	6	2	7	3	3	4	32
	Total	0	1	4	6	13	5	9	5	8	7	58
	1 min. +	0	2	0	2	1	2	4	3	2	3	19
K	30 sec. −	1	2	3	2	5	2	3	5	3	6	32
	Total	1	4	3	4	6	4	7	8	5	9	51
	1 min. +	0	2	3	5	9	7	9	9	9	9	62
Total	30 sec. −	1	4	7	9	14	7	16	14	14	19	105
	Total	1	6	10	14	23	14	25	23	23	28	167

The effect of repeated stimulation of the same spot is shown in Table IV, which gives, for the long and the short intervals and the totals of both, the number of times the cold spots failed to respond during every one of the 10 stimulations. The number of 'failures' increases with frequency of stimulation, and fewer 'failures' occur during the early stimulations than during the later. This result appears more clearly in the last three columns of Tables I and II, where the 'failures' are classified according to the time of occurrence as 'early'—(stimulations 2-4), 'middle' (stimulations 5-7), or 'late' (stimulations 8-10). The 'failures' are further classified in Table I for every interval, and in Table II for the long and the short intervals.

The distribution of the 'failures' among the different intervals shows nothing new—the differences are significant for *Dr* only; but the distribution in time shows that the 'failures' increase with the frequency of stimulation. The number of 'early,' 'middle,' and 'late' failures was 8, 18, and 32 respectively for *Dr*; 11, 27, and 20 respectively for *F*; and 11, 17, and 22 respectively for *K*.

The average number of 'failures' for all the *O*s is 10 during the 'early,' 20.66 during the 'middle,' and 24.66 during the 'late' stimulations. The difference between the averages of the 'early' and the 'middle' is 10.66; between the 'early' and the 'late,' 14.66; and between the 'middle' and 'late,' 4.74. The coefficients of reliability of these differences (D/σ diff.) are 3.13, 3.82, and 0.82 respectively. The first two, *i.e.*, those showing the reliability of the differences between the 'early' stimulations on the one hand and the 'middle' and 'late' on the other, are sufficiently high, we believe—since the differences would come in the same direction 99+ times respectively out of 100—to warrant the conclusion that something (adaptation or fatigue) has happened during the 'early' stimulations to make the spots less responsive.

TABLE V

SHOWING THE INTENSITIES REPORTED DURING THE FIRST 3 AND THE LAST 3 STIMULATIONS AND THE TOTALS OF EACH

O	Stim. No.	Intensity				Not Reported	Stim. No.	Intensity				Not Reported
		Int.	Mod.	Mild	Wk.			Int.	Mod.	Mild	Wk.	
	1	12	38	10	0	0	8	3	17	30	0	10
Dr	2	9	33	16	0	2	9	3	19	28	0	10
	3	9	26	21	0	4	10	1	25	20	2	12
	Total	30	97	47	0	6	Total	7	61	78	2	32
	1	5	33	22	0	0	8	0	13	42	0	5
F	2	2	24	33	0	1	9	0	13	39	0	8
	3	1	17	38	0	4	10	0	13	40	0	7
	Total	8	74	93	0	5	Total	0	39	121	0	20
	1	19	36	4	0	1	8	4	25	16	7	8
K	2	14	26	15	1	4	9	6	26	16	7	5
	3	15	24	16	2	3	10	6	20	19	6	9
	Total	48	86	35	3	8	Total	16	71	51	20	22

This conclusion is borne out by the difference in the intensities reported during the 'early' and the 'late' stimulations. As is shown in Table V, more 'intense' and 'moderate' colds, and fewer 'mild' and 'weak' ones were reported by all the *O*s during the first three stimulations of every spot than during the last three. Singling out the intensive extremes, we find that *Dr* reported 30 intense colds, 0 weak, and 6 'failures' during the first three stimulations, and 7 intense, 2 weak, and 32 'failures' during the last three; *F* reported 8 intense, 0 weak, and 5 'failures' during the first 3 stimulations; 0 intense, 0 weak, and 20 'failures' during

the last 3. *K* reported 48 intense, 3 weak, and 8 'failures' during the first 3; 16 intense, 20 weak, and 22 'failures' during the last 3. The number of repetitions is then (within the 5-sec.–5-min. limits of our experiment) of greater importance than the rate of stimulation in determining the responses of the cold spots.

Summary

The results of this study may be summarized briefly as follows:

(*1*) Every time-interval, short and long, gave, for all the *O*s, a large percentage of responses with cold.

(*2*) There was no agreement among the *O*s regarding the most or least favorable interval.

(*3*) For all the *O*s, the 3 long intervals gave more responses than the 3 short intervals.

(*4*) The difference between the average number of responses with the long and the short intervals was statistically reliable for all the *O*s taken together, but only for one of the *O*s taken separately.

(*5*) All the *O*s gave more responses of cold, and of greater intensities, with the 20-min. interval than with any other interval used.

(*6*) Intensity of cold tends, for all the *O*s, to vary directly with length of interval and inversely with frequency of stimulation.

(*7*) With every interval and for all the *O*s, repetition tends to reduce the number and the intensity of responses with cold.

(*8*) For all the *O*s, the difference in average number of responses between the 'early' and the 'middle' repetitions, and between the 'early' and the 'late' repetitions, are statistically significant, but not between the 'middle' and the 'late' repetitions.

Conclusions

We feel justified in concluding that the longer time-intervals are to be preferred when a high degree of correspondence between successive stimulations is desirable. We may, with confidence in the adequacy of the interval, repeat stimulations every 20 min.—consequently at least 4 stimulations may safely be made during an experimental hour: one at the beginning of the hour, a second after 20 min., a third after 40 min., and the fourth at the end of the hour. Tradition has, in the light of our results, needlessly erred upon the side of safety.

Stimulations may, however, be made safely even at a more rapid rate, for our results indicate that it is not the rate as much as the frequency of stimulation that conditions the response of the cold spots. Three stimulations can be made almost as safely at intervals of 1, 3, and 5 min. as at a 20-min. interval, for the number of times the cold spots failed to respond during the first three stimulations, when these intervals were used, is, as shown in Table IV, exceedingly small. The slight difference in favor of the longer interval may be more than made up by the increased accuracy, in locating and stimulating the same spot which the briefer intervals make possible.

APPARATUS

A MINIATURE MODEL OF YERKES' MULTIPLE-CHOICE APPARATUS

By Willis Beasley, Ohio State University

The multiple-choice problem has become a classic demonstration for most elementary psychology laboratories, although it still affords many research possibilities. The principle, first used by Hamilton when he placed his subjects in a small room having several doors used variably for escape,[1] was incorporated in a single and effective apparatus by Yerkes.[2] This apparatus was improved later,[3] and is now a standard piece in most laboratories.

Münsterberg as early as 1912 used the same principle with letters of the alphabet printed on cards.[4] Yerkes and Rossy employed a revision of Münsterberg's method, using circles and squares as symbols in place of alphabetic letters.[5] Burtt[6] and various others besides Yerkes have used the same attack on the problem-solving behavior of rats, pigeons, raccoons, pigs, dogs, monkeys, and apes. Brown and Whittell have reported results with human subjects.[7]

Yerkes' apparatus has a high degree of utility for elementary laboratory instruction; but where the sections of students are large, the problem of providing sufficient pieces of apparatus so that each pair of students may work simultaneously and separately is augmented by the cost of the product. Moreover, obtaining storing space for a dozen or more of these devices presents a serious difficulty for most departments. Zener and Wever[8] have described a cheaper device but one that is large and space consuming.

The writer has designed and built a number of miniature models of the Yerkes instrument which are inexpensive, small, and portable. Fig. 1 shows several views of the model. The dimensions of the box as shown in the closed views (A, B) are 16 × 10 × 6 in., and the weight is 8 lbs.

---

[1]G. V. Hamilton, A study of trial and error reactions in mammals, *J. An. Behav.*, 1, 1911, 33-66.

[2]R. M. Yerkes, A new method for studying ideational behavior, *J. Comp. Psychol.*, 1, 1921, 369-394.

[3]Yerkes, The study of human behavior, *Science*, 39, 1914, 625-33.

[4]Yerkes, *op. cit.*, *J. Comp. Psychol.*, 378 f.

[5]Yerkes and C. S. Rossy, A point scale for measuring the intelligence of adolescents and adults, *Boston Med. and Surg.*, 176, 1917, 564-573.

[6]H. E. Burtt, A study of the behavior of the white rat by the multiple-choice method, *J. An. Behav.*, 6, 1916, 222-246.

[7]W. Brown and F. Whittell, Yerkes' multiple-choice method with human adults, *J. Comp. Psychol.*, 3, 1923, 305-318.

[8]K. E. Zener and E. G. Wever, A multiple-choice apparatus, this Journal, 41, 1929, 647-8.

On the subject's side (D) of the apparatus, the functional characteristics of this design are exactly the same as those of Yerkes' model; the keys are considerably smaller, however, measuring 1/2 × 5/8 in. in cross-section and projecting only 7/8 in. when 'out.' The total key-board span with all members out is 13 1/2 in.

On the experimenter's side (C) of the apparatus, several modifications have been made. A rotary contact switch with contacts numbered in irregular order to eliminate spatial cues replaces the long row of jacks in the Yerkes model where spatial cues are easily obtained—and used consistently by human subjects. The detachable plug is A.C. input to the primary of an ordinary bell-ringing transformer. This eliminates the necessity for battery supply. The buzzer is relatively high-pitched and gives a pleasant sound. The row of lights used on the Yerkes model has been eliminated as superfluous. Since the span of numbered plates on the rear of the keys is only 13 1/2 in., the movement of the plate when *S* depresses a key can be used as effectively for a cue by *E* as a small light bulb. This eliminates another item of expense.

This apparatus can be used for experimentation in all cases where the Yerkes model is usable, and has the features of economy, convenience, and portability in addition.

AN IMPROVEMENT IN THE SEASHORE SERIAL DISCRIMETER

By Coleman R. Griffith and J. R. D. Eddy, University of Illinois

Among the precautions listed by Seashore for the use of the Serial Discrimeter is the following: "Occasionally persons will tend to press two keys at once consistently. This should be warned against. Observers must also be warned never to press all four keys at once except for winding, when experimenter will ask for it."[1] It has been the experience of the writers of this note that more subjects tend to press two or more keys than can be described by the word "occasionally." To use several fingers rather than one is the first reaction of most subjects and still others revert to it whenever they can. Needless to say, such reactions diminish the value of the records which the investigator may get from this apparatus. It is obvious that a method of preventing the depression of more than one key at a time would add greatly to the value of the Serial Discrimeter. This has been accomplished in the following manner.

The subject may press any one of four keys. These keys rest on the forward edge of a lever which is anchored some 12 cm. behind the apparatus. The levers slide up and down through the openings in the face of the apparatus. If, now, there is placed, just behind the face of the Discrimeter, the device shown in Fig. 2, each of the levers will, under certain conditions, be so locked that only one key can be pressed at a time. The details of construction are as follows. Two brass plates 4 in. long, 1 in. high, and 1/16 in. thick are cut according to the pattern suggested by the letters a,b,c,d,e,f,g,h, etc., in Fig. 2. Let these two plates be called M and N. Between M and N there is inserted a third plate

[1] R. H. Seashore, Stanford motor skills unit, *Psychol. Monog.*, 39, 1928, (no. 2), 62.

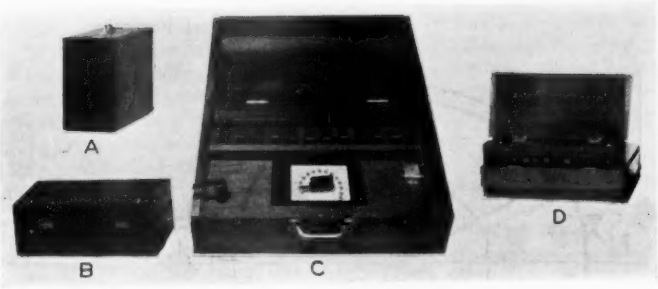


FIG. 1. A MINIATURE MODEL OF THE YERKES MULTIPLE-CHOICE APPARATUS

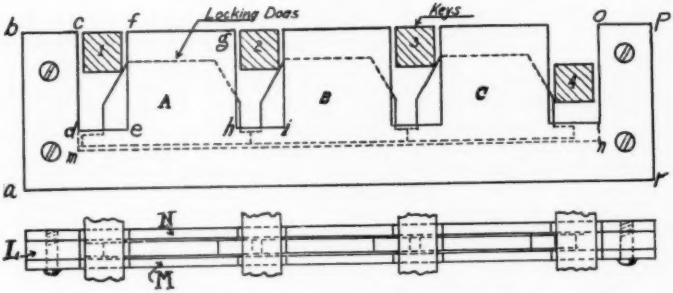


FIG. 2. LOCKING DEVICE FOR THE SEASHORE SERIAL DISCRIMETER

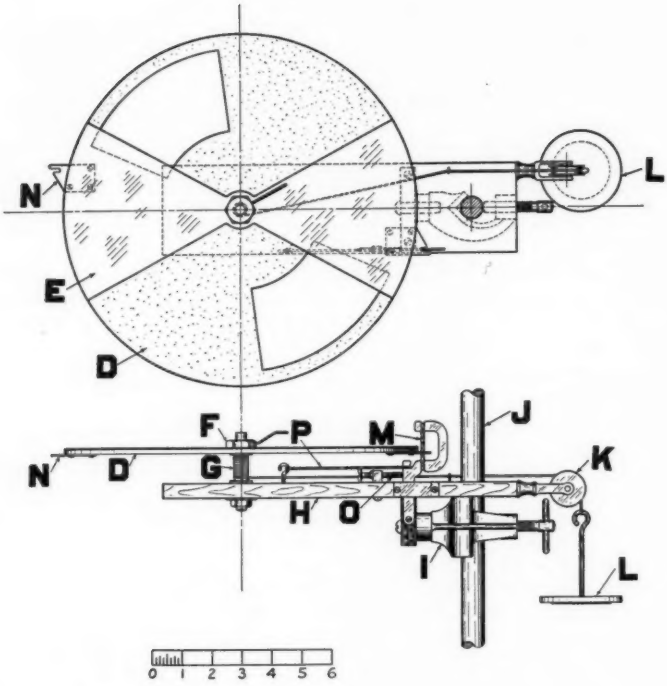


FIG. 3. PLAN OF SHUTTER FOR THE PROJECTION TACHISTOSCOPE
(The scale shows inches.)

of the same material cut in the form suggested by the letters a,b,c,m,n,o,p,r. Let this plate be called L. The letters A, B, and C represent dogs which are cut from the same material as are the plates M, N, and L. Since they, too, are 1/16 in. thick, they will slide back and forth in the slot created by the fact that plates M and N enclose the plate L. These dogs, as will be seen from Fig. 2, are so fashioned that they will lie adjacent to one another below the level that can be reached by the keys, as, for example, key 3. Their sloping sides make it possible to shove them aside by pressing a single key, a situation which is illustrated by key 4 which is shown in the depressed condition.

Fig. 2 presents two views of the facts just described. The upper part of the Figure shows the device as seen from the front. The parts of the dogs, A, B, and C which are concealed by plate M are drawn with dotted lines as is also the upper edge of plate L. The lower part of the figure pictures the device as seen from above where sections of the levers upon which the keys are mounted extend across the three plates M, N, and L.

The levers which operate the Discrimeter are 1/4 in. square. If the dogs are made so that they have a total clearance of 1/16 in. in the slot created by the three plates, they can be moved back and forth by any one of the keys so as to lock all of the keys save the one being depressed. Just underneath key 4 and adjacent to the letter 'n' will be seen the amount of clearance possible to the dogs, for the figure shows that the dogs are all displaced to the left by the action of lever 4.

It is obvious from Fig. 2 that keys 1, 2, and 3 could not be pressed so long as key 4 is down because key 4 has pushed dog 3 and all the other dogs to the left as far as they can go and thus thrown them in the way of keys 1, 2, and 3. When key 4 is released, it becomes possible to depress any of the other keys. After having released key 4, we will assume that the subject is to press down key number 3. In this case, the key would clear dog B and push dog C to the right 1/16 in. thus locking key 4. Keys 1 and 2 were already locked from the action of key 4. Now let us say that the next reaction calls for pressure on key 1. Dog A is thus pressed to the right and so, too, is dog B. Key 2 is locked by the action of dog A, key 3 by dog B, and key 4 by dog C.

The little device here described thus makes it impossible to press more than one key at a time. Any key may be pressed but the subject cannot beat the experiment by pressing all of the keys. Moreover, since the apparatus is so made that only one reaction is 'right,' the total effect of this device is to prevent the subject from making a greater score than his own skill would make possible.

A PROJECTION TACHISTOSCOPE

By Harold Schlosberg, Brown University

By arranging a simple rotary shutter to intercept its beam of light at the focal point, the Spencer Lecture Table Delineascope, Model B, may be made to serve as a very flexible exposure apparatus, especially for classroom work. This model of the delineascope has a flat hortizontal table, on which the slide to be projected is rested. Light passes through this slide from below, and after passing upward

through the objective lens, the beam of light is deflected from its vertical course by a mirror, and is thrown on a screen. Due to the open construction of this instrument, and to the fact that the slide is resting on a horizontal table during projection, it is very simple to manipulate slides and masks so that any desired portion of the slide may be projected. The shutter to be described below is simply a disk, mounted in a horizontal plane on a standard, and rotated by a weight. The disk is arranged to rotate across the beam of light just above the objective, so that apertures in the disk permit brief projection of the exposed material. A detailed description of the shutter, together with plans drawn to scale (Fig. 3), follows:

The disk (D) is of sheet metal. It has two segments partly cut out. These open segments may be partially covered by a cross-shaped shield of thin metal (E) which is held in place by a nut (F). The disk is accurately mounted on a spool (G). This spool is slipped over an axle, which is bolted into the baseboard (H). The other end of the baseboard is fastened by means of a clamp (I) to a 20 mm. support rod (J) with tripod base. The disk is rotated by a weight (L) hung from a string that pases over a pulley (K) and is coiled around the spool (G). The string should be of such a thickness and quality that it will wind on the spool in an even layer, with no overlapping. A short rod projects from the nut (F) so that the disk may be turned easily when rewinding the spool. If a constant pull is to be maintained on the spool the string must be short enough so that it will allow only one layer of turns to be wound on the spool.

The catches that control the movement of the disk are of some importance. A simple stud and bar arrangement was tried at first, but gave too much jar and rebound. The catch described below eliminates these difficulties. It consists of two parts, a latch (M,O,P) and two stops (N). The latch is cut from sheet brass, as shown, and is attached to the baseboard by a pivot screw and guides. It carries a cord (M) made of twisted rubber bands. This cord engages one of the ratchet-like stops (N) fastened to the under side of the disk. The normal position of the latch is determined by the set-screw (O) and the rubber band or light spring (P). The set-screw should be so adjusted that, when the apparatus is 'cocked,' the rubber cord of the latch rests fairly well down in the slot of the stop. When the latch is pulled back the rubber cord comes out of the slot, and the disk starts to turn. When the latch is released it returns to its normal position, and is ready to catch the other stop when the disk has made a half revolution, completing the exposure. The rubber cord of the latch is displaced by the angular portion of the stop, partially checking the disk, and finally falls into the slot. This prevents further movement or rebound of the disk, and leaves it ready for another exposure. With the dimensions given above, twelve or more consecutive exposures are possible with one winding.

The stand and baseboard must be arranged so that the disk is symmetrically placed with relation to the delineascope, and the opening in the disk is one inch past the narrowest part of the beam of light when the disk is cocked. This brings the exposure near the end of the half-revolution, when the disk is moving most rapidly. If the disk cuts the beam of light at the proper point, the image will come and go on the screen evenly, with no 'wiping.'

The length of the exposure may be varied by changing the size of the openings in the disk, or by using another weight. If the latter is done the screw (O) should

be readjusted, so that the catch will operate certainly, but quietly. Brilliance and size of the image may be changed as with any other projector. Where very long exposures are desired, as in memory work, two additional stops may be added, to hold the disk in the open position. The additional stops should be attached with thumb-screws, so that they may be removed quickly.

Since opaque material cannot be projected with this machine, the material must be prepared on glass or celluloid. The simplest method is to type the material on 'Radio-mat' lantern slides, but this gives rather small letters. A better method is to paste gummed black letters on glass plates. But if one wishes the preëxposure field to be of the same brilliance as the ground of the projected material, the slides must be prepared photographically. A negative plate made by photographing black figures on a white ground, if sufficient 'contrast' is obtained, is ideal, for it gives white figures on a neutral ground when projected. The same results could be obtained by using glass plates prepared with gummed letters as negatives, and printing them directly on lantern slides. A whole list of words may be prepared on one slide, and the single words projected as desired, by the use of masks. These may be of wood or cardboard, and should be placed under the slide. The mask should be one-half inch thick so that the slide will pass over a ridge that is placed at the rear of the slide table, to aid in aligning the slide when the delineascope is used for ordinary projection purposes.

The delineascope that is used with this shutter is not impaired in the least, so that it is available for ordinary projection purposes. Its open construction, flat slide table, etc., permit rather unusual uses. For example, the writer has demonstrated the 'iron wire nerve analogy' by placing a flat spiral of wire in a small beaker containing the acid. When the beaker was placed on the slide table a greatly enlarged shadow of the wire was thrown on the screen, in front of the class. Photographic records, as of eye-movements or action currents, may also be projected, if they have been taken on film or glass plates.

The arrangement described in this paper was intended chiefly for classroom work. Its simplicity, quietness, low cost, flexibility, and compactness recommend it for this purpose. The writer feels that these same characteristics would make it a valuable instrument for use in many research problems. If the bearings are carefully constructed and kept clean and well oiled, friction is reduced to a minimum, and the length of exposure is controlled by a number of constant factors, such as the size of the opening, and the pull of the string on the spool. The instrument may be calibrated by means of electrical contacts on the disk. Recalibration is rarely needed, since the disk is driven by gravity. Finally, it should be emphasized that with this type of exposure apparatus, there is no moving slide to distract the *O*, and that the image comes and goes by a uniform change throughout the field.

APPARATUS NOTES

A Pursuit Apparatus

In an attempt to determine the distance traversed by athletes engaging in the different competitive sports the following apparatus was developed. A playing field (gridiron, basket-ball court, or base ball diamond) was laid off to scale upon a light tin base, which was wired in series with a 4.5 v. dry-cell battery, an

electric impulse counter, and a small pursuit wheel mounted on a handle. At regular intervals on the circumference of the wheel insulation was placed, so that rolling it on the tin playing field caused a circuit to be made and broken for every unit distance. These makes and breaks were recorded by the impulse counter. By properly dividing the playing field with lines an observer can follow the path of a player with the wheel and determine by the number of contacts made the distance which he has traveled. The pursuit wheel, obviously, registers regardless of the direction of movement. The distances recorded in our observations were reliable. The average distance traversed during ten 4-min. periods of a number of basket-ball games was in every case more than ten times its average deviation. Different observers following the movements of the same player obtained comparable results.

It is believed that a similar set-up might be used advantageously in any situation where it is necessary to obtain data with respect to distance traversed. Such a pursuit apparatus is better adapted to measurement of this type than that involving a division of the floor into squares and then counting the number of squares crossed.

DePauw University STEPHEN M. COREY

AN INEXPENSIVE FINGER MAZE

Because of the necessity of equipping an elementary laboratory with limited funds we were unable to purchase mazes for class experiments on learning. We resorted to homemade ones, using a method of construction which may be of general interest. A base of three-ply veneer was used, into which were driven staples, one half inch long, standing shoulder to shoulder. The shoulders of the staples made a continuous elevated track which could be traced with the finger, after the manner with Miles' elevated finger mazes.[1] If reasonable care is taken to see that the staples are in actual contact, it is possible so to wire the maze as to administer punishment for errors. The constancy of width of the commercial staples makes possible an analysis of maze learning into its rhythmical as well as its direction and distance factors.

Each maze represented a cost of approximately twenty cents and one hour unskilled labor. They are strong, light, do not warp or crack, and have proved satisfactory from every point of view. The design of any maze may be copied.

DePauw University STEPHEN M. COREY

[1] W. R. Miles The high relief finger maze for human learning, *J. Gener. Psychol.*, 1, 1928, 3–14.

NOTES AND DISCUSSIONS

Absence of the Left Occipital Lobe without Impairment of Vision

The case reported here is that of a boy in whom the left occipital lobe of the brain had been absent apparently from birth,[1] and who was operated upon to be relieved of the effects of a porencephalon cyst. Before the operation, in response to the ordinary routine finger-test, and after the operation, as shown by very careful campimetrizing, he enjoyed perfectly good visual fields for both eyes, with no blindness whatever on the right half of the field of vision, such as would ordinarily be expected in the absence of the visual projection centers in the left occipital lobe. This case is significant in relation to the problem of cortical localization of function. If vision is localized as has been supposed, then this boy should have been completely blind on the right half of each field of vision. That he is not, as the charts show, leads to interesting speculation concerning the location of the substitute anomalous center, if any, and contributes to the growing mass of evidence in support of the view that even the relatively high degree of flexibility which we attribute to localization is not high enough.

The patient, an 11-yr. old white boy of good intelligence and lively demeanor, was brought to Dr. R. Glen Spurling, a neuro-surgeon of Louisville, with the chief complaints of seizures, nervousness, and headaches, frontal and temporal. His history showed that he was a forceps baby, with a skull opening in the occipital region, due to an undetermined cause. For two or three weeks after birth he had spasms, but with the improvement in the condition of his skull, the spasms ceased. With the exception of headaches, he was well from infancy until February 1930 when he had the first of a series of seizures. In these he lost motor control, but never fell or injured himself or lost ability to talk. The seizures varied in severity and duration, sometimes lasting as long as 30 min. He was always able to comprehend the onset of a seizure. He never frothed at the mouth or bit his tongue. Occasionally he was nauseated, and once lost control of sphincters. There were no jerkings of arms and legs, but these became drawn and tense. At the same time consciousness remained.

On one occasion after a seizure the right side of the body was numb. Ordinarily during a seizure his head was twisted to the right.His gait showed no disturbance, nor did his hearing, taste, or smell. To the rough finger-test, his vision appeared normal.

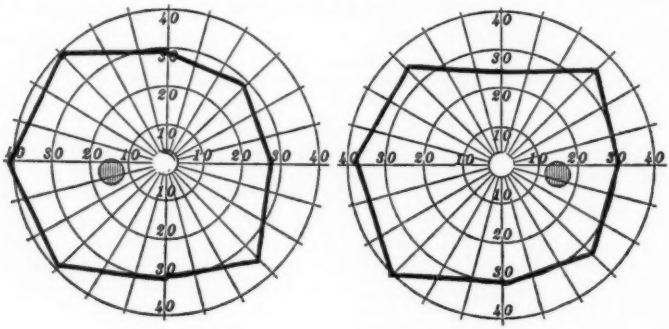
His optical disks were normal in color and size, with margins distinct, and retinal vessels normal. His pupils were abnormally large but responded readily to changes in intensity and in accommodation.

---

[1]The boy is left-handed as any one conversant with Dallenbach's and Papez's results might have predicted from the position of the lesion. Cf. K. M. Dallenbach, Position vs. intensity as a determinant of clearness, this Journal, 34, 1923, 285; K. M. Dallenbach and R. S. Burke, *ibid.*, 35, 1924, 267 f.; J. W. Papez and H. A. Cohn, A comparative study of the visuosensory or striate area in the two hemispheres of the human brain, *Amer. J. Phy. Anthropol.*, 14, 1930, 413 f.

On January 9, 1931 an encephalogram was made, which showed definite abnormalities. The right and left lateral, and antero-posterior films of the head, after injection of air, show a defect in the filling of the anterior horn of the left ventricle. On the antero-posterior films this ventricle contains half as much air as does the opposite side, and the defect is along the inferior and external lateral border of the ventricle. The left ventricle is very indistinct. On the lateral films the right ventricle is well-filled and larger than normal. The postero-anterior films show a large area of lessened density for the greater portion of the left side of the head. This area lies over the temporal, parietal, and occipital regions of the left side.

On the basis of the encephalogram and of the involvement of the right side of the body in the seizures, a diagnosis of left porencephalonic cyst was made, and



LEFT EYE RIGHT EYE

FIG. 1. SHOWING THE FIELDS OF VISION OF THE TWO EYES

an operation performed on January 11, 1931. A left occipital-parietal bone flap was turned. In the entire posterior half of the hemisphere there was a large cyst, with fluid completely covering the dura. The dura was reflected and found to be adhering to the brain (this adhesion was probably the cause of the motor disturbance) in the posterior portion of the postcentral region. *In the occipital region the cortex was completely absent over a space of 7 cm. diameter, 1/2 cm. thick and extending back of the parietal region.* A large choroid encephalon cyst had undermined it completely. When the cyst was opened, the brain stem, tentorium, veins of Galen and the choroid plexus were visible.

On February 7, 1931, less than a month after the operation, I made a very thorough and careful determination of his visual fields, using a large black campimeter marked out in concentric tangents. Fixation was at 1 m., monocularly, with the other eye carefully blindfolded with a heavy wad of cotton held in place by surgical tape. The campimeter was homogeneously, but not strongly, illuminated over its entire area of 3 x 2.5 m. The intensity of illumination was such that for my eyes, which are normal, the left gave a field of approximately the same extent as the left of the patient, and the right gave one symmetrical with that of the left.

I first tested him on eight meridians for each eye with a white stimulus-object 1 cm. in diam. The observations were repeated 5 times in succession for each meridian, and then those in the *right half* of the field were again repeated 5 times each for each eye, in order to insure the reliability of the results in the portion of the field in question. The patient was coöperative and intelligent, and maintained fixation, avoiding eye-movements, with surprising constancy. The results are given in the charts of the two fields. The field of vision of the left eye is normal in shape, being considerably more extensive on all meridians of the temporal side than on the nasal. The field of the right eye shows a curious distortion which is not characteristic of the normal. This is due to the very pronounced shortening of all the temporal meridians, and lengthening of the nasal meridians, as compared to the corresponding meridians of the left eye. In consequence the temporal half of the right field is less than it should be, and the nasal half is more by an approximately equal amount, and the fields as a whole are of about equal area for both eyes. Thus, the only discoverable abnormality is the distortion of the right field. But even with this condition, it is clear that there is for neither eye the loss of vision which would correspond with the absence of occipital tissue.

I thought it desirable to explore the nasal half of the left field and the temporal half of the right field for abnormal blind spots, which might conceivably correspond to the absent brain tissue. Nowhere was I able to discover areas of absolute scotomata with the 10 mm. stimulus-object, or areas of relative scotomata with a 2 mm. stimulus-object. Vision was clear and unequivocal throughout both fields, with the exception, of course, of the blind spots which were normal in situation and extent and are so indicated by the shaded circles on the charts.

It seemed also desirable to test the zonal sensitivity of his retinae to color—that is to discover the color changes which a constant stimulus produces in passing from periphery to fovea. This was done, and it was found that here too his vision was perfectly normal. A red stimulus-object of 10 mm., when brought in on any meridian from the periphery to the center, underwent the changes characteristic of the normal eye. It first became visible as black, then yellow, then orange, and finally at the fovea, red. Analogous results were obtained with green, blue, and yellow stimulus-objects.

Here to all intents and purposes is normal vision for the entire field of each eye, but with the normal cortical mechanism for the left half of each retina absent. The situation, aside from its intrinsic interest, invites speculation. The first explanation that occurs is that another cortical center has been established. To suggest where this might be, we must examine several possibilities. First, is it likely that the fibers from the entire retina pass to the right occipital lobe? This is not likely, because anterior to the cyst the structures seemed undisturbed. This means, then, that the fibers could pass through the chiasma, the superior corpus quadrigeminum, and the external geniculate body. Thus, we may suppose that up to this point the structures are normal, and that they simply have acquired an abnormal cortical projection area somewhere in the parietal region, instead of in the occipital. On the other hand, there may be no specific projection area. Perhaps the function of this area has in terms of *Gestalttheorie* found an equivalent in a set of dynamic brain relationships. The objection to this explanation, however, is that it is too illusive and cannot readily be represented by terms which have a tangible counterpart in our experience.

University of Louisville ELLIS FREEMAN

'The Cigarette Illusion:' A Reply to Dr. Wells

In a recent note, Dr. Wells has catalogued Helson's cigarette illusion as "merely a case of contrast."[1] Dr. Wells has not only failed to meet Helson's objections to an explanation by contrast, but she misinterprets, I believe, the further facts which are introduced in her note. Her exposition, moreover, contains several misstatements of fact. Finally, a few simple experiments, which I shall propose, will prove conclusively that Dr. Wells' explanation by contrast is inadequate.

(1) Dr. Wells has not properly applied Helson's pinhole test, one of his experimental tests of contrast.[2] Her error lies in the fact that she has "allowed a background of red to reach the eye." "Helson's method," she says, "was to look at the glowing tip through a pinhole in a white card."[3] Helson did not say this. If she saw it demonstrated at Bryn Mawr in such a manner it was by accident. Helson's test was to look at the tip through a hole in a *black* card. If Dr. Wells, after having first produced the illusion, had tried this test, she would have found that the illusion persisted and contrast would have been ruled out.

(2) Dr. Wells disagrees with Helson and states that "spectroscopic analysis of cigars and cigarettes shows green to be the predominant component."[4] This is not true and Dr. Helson's statement stands. It is apparent that Dr. Wells has based her conclusion erroneously on the width of the spectroscopic band which bears no relation to the intensity or predominance of that hue in the composite light. The fact that green extends over half of the visible spectrum is a function solely of the wave length of the light and the nature of the dispersing prism.[5] Dr. Wells' error on this point biases her subsequent discussion and should therefore be corrected.

(3) The spectroscopic analysis of the color is used by Dr. Wells to explain the anomalies of the cigarette illusion. "The change is more marked under red illumination. This difference may be adequately explained in terms of the physical composition of the lights."[6] What has the physical composition to do with contrast? As far as the laws of contrast go, a red light is red and a yellow light is yellow regardless of their physical properties. To the best of our knowledge no writer on contrast has ever referred to spectral composition to account for the effects. By resorting therefore to the physical properties of the stimulus, Dr. Wells is admitting on the face of it that the cigarette illusion is more than contrast![7]

(4) Dr. Wells is inconsistent and ambiguous in her use of the phrase, "impurity of the lights," as an important condition of contrast. When referring to

---

[1] E. F. Wells, Dr. Helson's 'cigarette illusion,' this Journal, 43, 1931, 136-139.

[2] H. Helson, A new visual phenomenon—the cigarette illusion, *Psychol. Rev.*, 37, 1930, 275.

[3] *Op. cit.*, 137. [4] *Ibid.*, 136.

[5] The true distribution of energy from the cigarette may be obtained very closely by finding the energy distribution of a black body radiator of the same color temperature.

[6] *Ibid.*, 136.

[7] We are using contrast throughout our discussion, in the sense usually employed, to mean the induction by a given stimulus of the opposite effect in the adjacent unstimulated area.

the cigarette, the large amount of green accounts for the *heightened* effect and in the very next paragraph she uses the impurity of the light as a reason for *lessening* the contrast effect. We do not deny the facts that Dr. Wells reports but we insist that a reasonable and consistent explanation be given. If the impurity of the light is important it requires a logical explanation which is neither proposed by Dr. Wells nor offered by the contrast theory.

(5) "Since the contrasting color tends to appear," says Dr. Wells, "it would seem that this might be regarded as a contrast phenomenon."[8] Adaptation or negative after-effects could equally well be accepted on this same ground. Dr. Wells merely describes the effect and slips into the logical error of using the description as an explanation.

(6) The test upon which Dr. Wells rests her explanation violates the essential conditions of the illusion. She sets out to "eliminate the red background absolutely" and claims to have attained a "rigid elimination of reflected red light."[9] Under such conditions, we must conclude, the observer could not tell whether red light was present or not. Can Dr. Wells say fairly then that the cigarette had been observed under red illumination, the *sine qua non* of the illusion? I am admitting the fact that without the customary contrast conditions the effect will not appear. This might have been inferred, however, from Helson's original discussion. It did not require Dr. Wells' experiment to establish it. Consequently, the conclusion on the basis of this fact that the effect is due to contrast is gratuitous. Judgment must be made on the basis of other evidence, which, as I have shown, is unfavorable to the contrast explanation.

(7) A single experiment will prove conclusively that the effect is more than contrast. An orange color was produced which was judged by a number of *O*'s to be equivalent to the cigarette tip.[10] This color was substituted for the cigarette and was viewed under as nearly identical conditions as possible. But, whereas the cigarette turned green under red light, our new color became only slightly more yellowish and did not approach green at all. On the basis of the contrast theory, the different behavior of the cigarette and the new light could not have been predicted. The colors were unlike solely in their physical compositions, a factor which is often indistinguishable to the eye. An explanation based on contrast is clearly inadequate.

(8) Dr. Wells states that "red adaptation plays no noticeable part in the effect."[11] That this is not true may be demonstrated in a very simple experiment. If one eye is exposed to red light for a period of 20-30 sec. and then the cigarette tip is viewed first with one eye and then with the other, it will seem strikingly green to the red adapted eye and normal to the other. This effect will last nearly 2 min.

(9) The ambiguity of the concept of contrast is apparent when it is held to cover such a case as Dr. Wells reports when one red light, viewed in the presence of another red light, becomes orange. Why should anything in contrast with red become orange and least of all a red light? Is orange in the "direction" of the

[8] *Op. cit.*, 137. [9] *Ibid.*, 138.

[10] This light was produced by superimposing two Wratten filters, Nos. 25 and 58, in front of an electric light. Of this light, 60% lay between 595 mμ and 615 mμ.

[11] *Ibid.*, 138.

complementary green as Wells implies? It might be a brighter red or a darker red, or it might be a more or less saturated red, but red in contrast with red is always red unless it is so pale as to become slightly green. If anything were to be orange it should have been the cigarette tip which was reddish orange to start with.

In conclusion, I believe that the facts reported by Dr. Wells may be resolved into (1) those that are implied in Helson's discussion adding nothing new to our understanding of the illusion, and (2) those that bear no logical relationship to the concept of contrast, making it obvious that the phenomenon is more than contrast. I have shown in this note that the explanation by contrast is inadequate and predictions based upon it are ambiguous. I cannot agree, therefore, that the phenomenon is to be labelled as "merely an instance of color contrast."

Bryn Mawr College EDWIN B. NEWMAN

MORE ABOUT THE 'WOLF CHILDREN' OF INDIA

In 1927 P. C. Squires published a short article which outlined correspondence received by him regarding the so called wolf children of India.[1] The current note represents an attempt to check further upon the lives and activities of these children and to obtain additional evidence of the authenticity of earlier reports.

Dr. Squires received word directly from the Rev. J. A. L. Singh, a Christian missionary at Midnapore, India, into whose household the children were taken and who with the assistance of his wife undertook to educate them. The Rev. Singh's letter stated that the children, one of whose ages was estimated at about two years and the other at about eight years had been found living in a cave with wolves in an outlying and sparsely settled region. Their general behavior at the time of capture in the fall of 1920 was much like that of animals and later attempts to civilize them proved in the main to be futile. Apparently they did not come under the charge of the Rev. Singh until about two weeks after they had been apprehended.[2]

Our present information comes from Professor E. W. Mumby, an American psychologist and athletic director in Lucknow Christian College, Lucknow, India. Mr. Mumby was unable to visit Midnapore and is somewhat dubious about the possibility of obtaining 'actual facts' although he writes that "there is no doubt about the existence of the wolf children." He encloses a letter from the Rev. John A. Howard of Midnapore, dated January 22, 1931, which gives a brief account of the stage of development attained by these children and of their ultimate death. Mr. Howard's quoted statement of the seizure of the 'wolf children' in words which purport to be those of their captor (whose name is not given) is as follows:

Eight years ago I was on tour of my villages. One evening one of my people said, "There are ghosts in the woods. We are much afraid." So I watched the next night. Just before sundown, three old wolves came up out of a hole. Then two cubs and at last a queer human-like animal. The man with me wanted to shoot. But I forbade him. No one would go near the den so I got men from seven miles distant who knew not of the ghosts. They dug out the den. Two

[1] P. C. Squires, 'Wolf children' of India, this JOURNAL, 1927, 38, 313-315.

[2] *Ibid.*, 314.

wolves ran off, the third was killed with an arrow. She being the mother refused to run away. Down in the bottom of the den cuddled up with two furry cubs were two queer little girls devoid of clothing. Against their desires we gathered them up and brought them home. Whether they were illegitimate and thrown away or torn from their mother's cottage door by the wolves, we do not know. For the first four or five years they ran about on their hands and knees. After being compelled to, they could stand but not run. The younger never learned to talk, could only grunt and growl. The older one lived to be a little over six years of age with the mentality of a two and one-half year old child. She learned to talk and developed a vocabulary of about one hundred words. They never asked questions. Their jaws were large and square supposedly from gnawing bones. Both have been dead for some time.

It is to be remarked that this account differs from that received by Dr. Squires in several significant details.

Indiana University W. N. Kellogg

The Twenty-Sixth Annual Meeting of the Southern Society for Philosophy and Psychology

The twenty-sixth annual meeting of the Southern Society for Philosophy and Psychology was held in the University of Virginia, Charlottsville, on April 3rd and 4th, 1931. The program on Friday forenoon was largely experimental. After the words of welcome, spoken by the President of the University, the late Dr. Edwin A. Alderman, the program began with a paper by Lyle H. Lanier (Vanderbilt University) on "Speed of Reaction at Different Levels." In the first series of experiments the reaction-times of 17 different responses were intercorrelated. Here are some of the results: mean between different pairs of simple reactions to tactual, auditory, and visual stimuli, 0.87; discrimination and choice reactions, 1.00; mean between different associative responses (free, partially controlled, and fully controlled), 0.83; mean between simple and discriminative reactions, 0.44; mean between the simple and the discriminative and choice reactions, on the one hand, and the three associative reactions on the other, practically 0.00; and mean between the different pairings of form-naming, color-naming, reading names of colors and of forms, 0.47. Serial manual-movements, cancellation, substitution, and simple and complex card sorting, were also used. In general, the correlations indicate that "where the 'set' and effector mechanisms are common the correlations between speed measurements will tend to be high; the greater the postural and effector diversity in the two reactions, the lower the correlations between speed measurements." In another series of experiments (50 reactions from every *S*), involving the measurement of the speed of reactions integrated at three different levels—simple reaction (cortical level), lid reflex (cerebro-spinal reflex), and galvanic reflex (autonomic reflex)—"all of the correlations were low and unreliable." Results indicate "that conditions determining rate of response over these different neural arcs tend to vary independently." None of these latter response types showed any correlation with scores on the Alpha test. The *S*s used in all these experiments (13 graduate students) constituted a rather homogeneous group. Results on larger groups at various ages, embodying diversified responses at different levels would be valuable.

Buford Johnson (Johns Hopkins University) reported a second study, made in coöperation with Miss May Wilson, on the variations in finger pressure in

relation to complications of response. Twenty children were selected as subjects in the present study, but the pressure-stylus was not sensitive enough for those under 2½ yr. of age, and certain older ones were unable to complete their records, so the number of cases was inadequate. (The study is still in progress.) Three degrees of tapping performance were obtained, *S* in all cases standing during the test: (1) simple tapping only on one plate, 1 min.; (2) 'complicated tapping' alternately on the two plates, 1 min.; (3) same as (2) but lasting 2 min. Records of tapping on each board and of pressure were automatically recorded on a kymograph drum over a 1-sec. time-record made simultaneously with a Jacquet chronometer. Pressure curves were measured by means of a planimeter in terms of average height for 10-sec. periods. In general the children showing intital high pressure curves followed by gradually decreasing pressure were the more efficient. Some *S*s with continued high pressure maintained their efficiency. Initial low but increasing pressure tended to be associated with low efficiency. Degree of efficiency showed no correlation with I.Q., and it decreased, as would be expected, with increased complication of movements and with temporal duration. Several *S*s deviated noticeably from the general tendencies. More work is necessary to find the real basis of pressure and efficiency differences, whether innate or environmental. It is well known that high tensions (not mere finger pressure) are present in initial stages of complex learning adjustments.

Wayne Dennis (University of Virginia), considering the non-random character of initial maze behavior, reported observations on the behavior of 59 white rats which were allowed the freedom of an elevated simple alternation multiple-T maze during a restricted period of time. No preliminary feeding on the maze was permitted. "Two of the rats went from entrance to exit of the maze without a single error and without retracings." The chance of a rat's doing this was reported to be about 1/23,000,000. All of the active rats made fewer errors (including return runs) than would be expected on the laws of chance. The data show clearly that initial maze-behavior need not be random and that it is not always characterized by excess movements. Dennis argued that the avoidance of errors as well as their elimination is a problem in maze adjustment. One cannot, of course, expect exact conformity with chance expectations unless all experience effects are controlled from the first move onward. This control seems to be impossible. The data given are not sufficient for criticism on this point. It is not impossible, even on chance factors, that two of the rats should make no 'errors' the first run. If errors are defined as in an earlier study by this investigator, one is not surprised even on initial approaches to a bifurcation to find fewer errors than expected by chance. The various possible cues must certainly be considered also. 'Excess movements' may be grossly considered as wrong directions at a bifurcation, or they may be finer mal-coördinations. Unfortunately data for consideration of these matters were not given. Prevention of all 'errors' (grossly defined) on initial runs, one might infer from indirect data available, would not eliminate later maladjustments. A thorough, well-controlled experiment on this problem would possibly be illuminating.

Karl C. Pratt (Ohio State University) made a general report of progress in the now well-known investigations on the behavior of new-born infants by objective methods as formerly published in a monograph by himself, Nelson and Sun. By means of self-recording instruments the activities of the infant under con-

trolled conditions are recorded and described in their true temporal relationships. Studies have been made not only of general activity aroused by internal stimuli, but also of responses to various external stimuli with irrelevant factors controlled as far as possible. Special studies have recently been made of the sucking and swallowing reflexes, and attempts have also been made experimentally to establish habits at this particular state of development. Results to date seem to point toward the view that early reactions are highly generalized and become particularized with practice rather than to that which would build the larger coördinations from smaller units. The objectivity in this study apparently owes much to A. P. Weiss, who was originally one of the two supervisors of the work.

R. C. Davis (University of Virginia) reported results of an attempt to measure individual and group differences in certain simple physiological characteristics supposed to be less affected by differences in cultural conditions than are mental tests. The latent period of the Achilles tendon-reflex was chosen as an index of the velocity of the nerve impulse over a simple arc. This velocity, he supposed, would be little influenced by general education or specific training and would therefore be determined largely by innate factors. The method of measuring the reflex time by means of a cathode ray oscillograph apparatus has been published. Four groups of *Ss* were used—white and negro college students, and white and negro feeble-minded adults of approximately the same age as the students. In every group the sexes were equated. An average of about six reflex responses was obtained from every *S*. Small and unreliable differences appear in the mean latent times, but correlations of these latent time-periods with height of the *Ss* were found to be 0.64 in the white and 0.83 in the negro students, and 0.53 and 0.55 in the respective feeble minded *Ss* of the two race-groups.To control the height factor, therefore, every *S*'s height in inches was divided by his latent time in m.s. (milliseconds), giving the ratio inches by height/m.s. The means of these ratios were 2.004, 1.904, 1.806, and 1.801 for (1) the white and (2) the negro students and (3) the white and (4) the negro feeble-minded, respectively. Differences are reliable between groups (1) and (2), (1) and (3), (1) and (4), (2) and (3), and (2) and (4), the difference in every case favoring the first group in each pair. There was no appreciable difference between (3) and (4). There are, of course, as Davis admitted, several assumptions made in this study which must be further investigated, as well as the relations between speed and intelligence as measured by suitable tests, before conclusions regarding innate race-differences are justifiable. The speed differences between whites and negroes and between certain other racial or national groups have usually been the most marked and reliable in a considerable number of investigations that have been carried out, but exactly similar differences have been found between samplings from different social environments of practically identical race combinations, indicating that speed is probably a cultural factor.

Willis Beasley (Ohio State University) reported experiments on the discrimination of periodic loudness-variations in mistuned binary chords. It is well known that Helmholtz's explanation of beats is interference of vibratory motion in the basilar membrane, whereas "the Young-Smith theory assumed that the compound wave-form changes due to phase-drift is the basis of beats." It is not easy to secure reliable objective evidence on this problem, although a number of indirect methods have indicated the improbability of direct discrimination of

compound wave-forms simply on phase changes in the binaries. Lord Rayleigh, on the assumption of a resonance theory, held on mathematical grounds that such discrimination is impossible. Beasley holds that he has demonstrated experimentally that phase-change is discriminable in the case of the ratios 1:3, 1:2, and 2:3, but not with 3:5, 5:8, and 9:16. The former three cases show trigonometrically maximum amplitude-changes (irrespective of sign) of the resultant amounting to 25%, 12%, and 4% respectively, for phase displacements of the h-components having the magnitude $90°/l$ (h here indicating the higher and l the lower frequency ratio of the binary complex). The "component l x h" was measured and found to be sufficiently sub-threshold to be "irrelevant for hearing."

These results seem to be subject to the criticism that the effects of the characteristic combination-tone frequencies 1:1 (slightly mistuned and therefore making beating possibilities) may be responsible for the results in the first three intervals. In the last three such nearly unison frequency-ratios of the characteric difference tones are practically impossible in any appreciable intensities, even though, as is well-known, combination tones may be multiples of the primary tones (*e.g.* $2\,l$, $3\,l$, $2\,h$, $3\,h$), and therefore not easily perceptible, as well as differences or sums (*e.g.* $h - l$, $h + l$, $2\,l - h$, $3\,l - 2h$), as mathematical theory shows. It has long been known that beating effects (periodic increase and decrease in intensity) of inaudible characteristic difference tones (those of ratio 1) of slightly mistuned primaries are ascribed to the latter tones.

J. E. Wallace Wallin (Atlantic University) outlined as the "prime qualifications" of a director of a clinic for the guidance and training of maladjusted school children, "fundamental grounding in the social sciences, and particularly in normal, abnormal, clinical, experimental, applied, genetic, and educational psychology, in tests and measurements, in child study, in elementary, special and remedial methods of education, in mental, social, and educational pathology, and in mental hygiene." He contended that in all these lines except "mental hygiene and mental disorders" psychiatrists are amateurs instead of experts," and that with as thorough training in their specialties as physicians now have in medicine "clinical psychologists and clinical educationists. . . . are fully as competent as psychiatrists to direct clinics dealing with the training of maladjusted children." The preference now accorded physicians he attributed to their 'selfish propaganda and blind prejudice.' He regarded it as important that psychologists assert their own qualifications for such work and justify their assertions by adequate preparation. But certainly statements like the above as to directions and extent of "fundamental grounding" need to be carefully supplemented by definitions and specifications as to amount of training, probably in terms of essential techniques in which the director should be trained as well as in extent and nature of experience. The several fields enumerated are not clearly distinct.

Friday afternoon was devoted to philosophical and other theoretical papers. Anna F. Liddell (Florida State College for Women) presented, in her own translations from the original, the knowledge of God as expounded by Nicolaus Cusanus (1401-1464) in his *De Docta Ignorantia*.

H. M. Johnson (University of Pittsburgh), discussing the logical structure of systems of psychology, held that the form which any description may take depends upon one's arbitrary choice of a set of rules which may be regarded as

axioms. These axioms must first be so constructed that they satisfy every proposition in the system. "Amongst the offerings" available he gave the propositions (a) that "Mental content is not exhibitable" to another person, (b) that "All representables are exhibitable," and (c) that "Mental content is representable." Only (any) two of these may be chosen, "for any two may be used to disprove the other one." Thus (a) and (b) "can be used as the foundation of a strictly objective psychology," since such a psychology "need not deny the existence of sensation and feeling as facts." A subjective psychology may rest on the first and third propositions; "but the necessary rejection of the third (proposition) would invalidate the usual treatment of conditional responses, learning, association, and rational and imaginal processes." He held that the frequent self-contradictions by the authors of most of the conventional psychologies resulted from their attempts to treat all three of these propositions as axiomatic. But, have we not learned from older 'systems' of psychology the dangers of starting from formally accepted axioms or propositions?

Charles K. Davenport (University of Virginia) presented a paper on "The Problem of Organization in the Living Organism and Its Bearing on Philosophical Principles." He considered the general characteristics of electro-chemical organization in a living organism as revealed particularly by the researches of L. J. Henderson in physiology. This type of organization was contrasted with that found in the physical world on the inorganic level, the former representing heterogeneous dynamic equilibria made up of processes which run in cycles to repeat themselves. It was pointed out that the intimate relations of a living organism with its environment are essential to explain its peculiar type of organization. From the basis of this survey Davenport made critical estimates of current philosophical theories as to the nature of the organism, vitalism, teleology, emergent evolution and mechanism.

Fritz Marti (Goucher College), discussing Schelling (1775-1854) in 1795, traced Schelling's 'Absolute I' back to the problem of the unconditioned in Kant's critical philosophy. The 'I' cannot be thought of unless it thinks itself, or *is*. No proof can be given of the absolute, for that would bring it into the realm of what is dependent. It is this non-dependence of the 'absolute I' that gives it its freedom. It is not an object of thought because that would give dependence: it is not *fact* but *act*.

William S. Weedon (University of Virginia), in a paper on "The Doctrinal approach to Scientific Theory," pointed out that the notion of "doctrinal function," introduced into modern logic by Cassius Keyser and given wider application in R. D. Carmichael's *The Logic of Discovery*, gives a new meaning to the medieval distinction *intentio prima* and *intentio secunda* "since doctrinal function and genuine doctrine stand to each other as logical form to the exemplar of that form." Weedon held that "the fecundity of this method of approach to problems of scientific theory is evident from the fact that we are led at once to the formulation of the new laws which can be verified at will." He cited progress in economics as an example and said that in physics "theoretical mechanics constitutes the doctrine of which the systems of pure mathematics are the corresponding doctrinal functions." Is not this alleged fecundity an illusion? One wonders if a good case could not be made out for the view that science has been progressive directly in

proportion to its attempts to settle experimentally the problems and hypotheses which have grown out of the contradictions and inconsistencies in its own data.

Peter A. Carmichael (University of North Carolina), discussing the relations of scientific law to logic, challenged the alleged permanency of law, a characteristic usually ascribed to it and held not to be due to the data of natural science but to logic, and specifically to the principle of identity. The patterns, or laws, of phenomena change with the phenomena themselves, so laws are not in reality constant. The discrepancy between logic and phenomena is apt to encourage the belief that law is either a creation of its discoverer or a mere convention. The conclusion was drawn, however, that the rational and phenomenal worlds "are ultimately one by reason of the fact that there are no finite individualities but only one individuality, the total universe of being. Law and logic are here identical and are continuous with phenomena." It must be confessed by a layman that 'proof' by this method is not very convincing in our scientific age. Are all phenomena, then, harmonious and unitary in spite of appearances? Is the 'proof' not merely a bit of circular reasoning?

At the conclusion of this session the members of the Society were delightfully entertained at tea in the Colonnade Club.

The subject of the presidential address, by Ethel Bowman, following the Society Dinner at the Farmington Club on Friday evening, was "Belief: A Problem for Psychology." Dr. Bowman maintained that the investigation of belief had of late been left largely to philosophy and religion, whereas both from the introspective and the behavioristic point of view there are aspects of belief which are essentially psychological: the experience of believing as such, independently of the truth or falsity of the belief (introspective aspect), and the determination of behavior by beliefs. (It may be appropriate to suggest that from the latter point of view belief *is* behavior and like all other behavior is a function of certain stimulus situations.) The confusions and ambiguities in the term 'belief' were indicated and results of certain introspective studies of belief as acceptance or rejection of a proposition were reported. Neither a distinct 'belief feeling' nor a 'not' feeling (in disbelief) were revealed, although when the contents were congruent with the statement the response was affirmative; when not so, negative. No affective tone was consistently present. Different stages of the experience ranging from simple mechanical acceptance or denial to doubt were reported, vacillations with unpleasantness appearing in certain stages of decreased subjective assurance. Doubt was always affectively toned. The conclusions were, (1) that certain beliefs and disbeliefs *may* reveal affective tone but that doubt is rightly placed with the sentiments, and (2) that in certain objective psychologies belief should be treated in connection with the motivations, such as integration of reflexes arising from internal stimuli of hunger, thirst, or sex, and built up under environmental conditions into stable habit-systems.

The program on Saturday morning, following the business meeting, was opened by Roy M. Dorcus (Johns Hopkins University), who made a preliminary report on results of an experiment on continuous rotation of the albino rat. Griffith (1922) had reported certain inborn turning tendencies, muscular disturbances and eye-movement effects on the non-rotated progeny of rats rotated at various speeds and periods of time. Griffith's originally rotated rats had also manifested various muscular incoördinations, disturbed ocular movements, and rather persistent

tendencies to walk or run parallel to the direction of rotation. Dorcus is seeking to check these experiments in as far as he can ascertain Griffith's technique, and to eliminate acceleration and retardation factors, and extreme centrifugal force, which might have destructive effects on the membranous canals. He is using a rotating case that is 4 ft. in diam., having sides of sheet metal and top and bottom of 1/4 in. wire mesh. Three inches from the circumference is a partition, dividing the cage into two compartments—a central cage 3 1/2 ft. in diam. and an outer ring-like cage 3 in. wide. Animals are fed, watered, and, when desirable, removed without changing the speed of rotation which is controlled by a synchronous motor. Thus far 12 animals (6 in each compartment) continuously rotated (speed not given) in the clockwise direction for 8 weeks have shown no peculiar behavior after a "few hours" of initial adaptation. These animals were about 120 days old at the beginning of the experiment. Two 'test animals' (one from each compartment) were taken out weekly for observation and returned, and at end of 8 weeks the rotation cage was stopped. The test animals acted no differently in the rotating cage than those which were never removed. All animals had a tendency in the cage to run in a clockwise direction "for a few minutes" when rotation ceased; when removed and placed on the floor no disturbances were observed. The two groups of animals (subjected to different centrifugal force) showed no differences in behavior.

Joseph Peterson (George Peabody College) presented results of a statistical analysis of the development of skill in dart throwing by 10 sixth-grade boys (mean age 147.00 ± 1.38 months) each of whom made 300 throws in series of 20 each half-week period. The chief problem was to determine variability coefficients in relation to the size of the standard deviation. It is obvious that wholly unreliable measurements will fail to reveal individual differences, that true standard deviations would be 0.00 and all means equal. The formula for the true standard deviation is $\sigma_t = \sigma_{obt} r^{\frac{1}{2}}$, where r is the reliability coefficient. The scores used in the present analysis were means of cm. errors of successive groups of 10 throws, there being therefore 30 such scores for every *S*. These scores were divided into three series of 10 each—an initial, a middle, and a final series. The mean and the σ or each of the successive scores were found, and all scores were then stated in terms of the successive σ-units, so that any changes in the size of scores with practice would be eliminated, the means all becoming 0 and the standard deviations unity. Correlations between any two sets of scores by the 10 *S*s could then be found simply by summing the products of the standard scores by the several *S*s and averaging ($r = \Sigma x y/n$). Correlations (reliability coefficients) were found to vary considerably in the first series of 10 successive scores and much less in the second and third, but the greatest reliability was found in the middle series. The mean of the first 10 coefficients—r_{12}, r_{23} ..... $r_{(10)(11)}$—was 0.16; that of the second 10—$r_{(11)(12)}$, $r_{(12)(13)}$, ... $r_{(20)(21)}$—was 0.55; and that of the last ten—$r_{(21)(22)}$, $r_{(22)(23)}$, ... $r_{(29)(30)}$—was 0.48. In terms of standard units, the 'true' standard deviation of the initial series as predicted from the above formula ($\sigma_t = 1\sqrt{.16}$ was 0.40; that of the second, 0.74; and of the third, 0.69. This shows that the percentage variability (σ/m) of the obtained scores, calculated without finding the true standard deviation, would be very much too large as compared with that of the middle more reliable series of measurements, and might easily give a false notion as to the extent of increasing variability with practice. The

σ_t by the method of finding the σ of the means of the several scores by each S in the first series, was found to be 0.35; those of the middle and final series, respectively, were 0.68 and 0.75. These are reasonably close approximations to the 'true' standard deviations as predicted from the formula, and when applied to the raw scores gave coefficient of variation (σ_t/m) of 0.213, 0.248, and 0.248 for the successive three divisions of the learning process. Even with these corrections of the σs, the Ss seem to have become more different with practice. (It is, of course, obvious that the means need no correction since they have no constant tendency to change in value with increasing reliability of the scores.)

Max F. Meyer (University of Missouri) illustrated with a large model, functioning hydraulically, the operation of the basilar membrane as he conceives it to function in hearing. The basilar membrane was represented by a normally tension-free partition which was soft, flexible and slightly elastic. His view was essentially that presented and illustrated in the Lexington, Kentucky, meeting two years ago, but there were some improvements in the apparatus. There was no discussion. The model, of course, differs radically from the real cochlea, and the movements demonstrated are very slow. It is obvious that the model as constructed could function only in the manner shown.

Knight Dunlap (Johns Hopkins University) reported work on the construction of new learning apparatus and compared the rote learning of maze patterns and paralog series. Difficulties lie in the way of comparing the usual learning of a series of 'nonsense syllables' (one syllable paralogs) with maze learning; (1) the syllables series is by rote and no 'errors' are possible except in reproduction, whereas maze learning is by trial and error and all errors are made while learning; (2) all the units in the syllable series are different, while in the maze series similar units are repeated; and (3) time is determined for S in the syllable series, but in the maze learning time is mainly under the learner's control. These differences invalidate comparisons of records by the two methods. Hence block-pattern finger mazes have been devised with true pathways only, and paralog series of only four different paralogs in each series have been constructed to parallel the several maze patterns used. A 'memory apparatus' has been made which is motor-driven but so controlled by the learner that he advances the series simply by a tap on the key. The apparatus may also be controlled by a clock-contact, and will repeat a series of any length as many times as needed without resetting. For comparisons with trial-and-error learning the patterns used in the true-path mazes have been constructed in other mazes having blind alleys. In the rote learning a minimal number of repetitions is allowed before the first attempt at reproduction, and an attempt at reproduction is required after each further repetition, thus affording better control of attempted repetitions. In the true-pattern mazes, reproduction is made in an open-pattern maze which allows a possibility of three errors at each step. Work on the construction of a sufficiently large list of two-syllable paralogs out of the consonants *b*, *d*, *f*, *g*, *k*, *l*, *m*, *n*, *p*, *r*, and *s*, and the five vowels, is in progress, with a view to getting for all persons a sufficient number of syllables without obvious associations.

Frank A. Geldard (University of Virginia) reported a check experiment on his earlier work on the effect of peripheral stimulation on foveal sensitivity. He had earlier found that the facilitation of foveal senstivity is a logarithmic function of the peripheral stimulation. The check experiment was motivated by recent re-

ports in the literature which seemed to indicate defects in the original formulation. Since these reports were concerned with sensitivity as measured by the critical frequency of flicker, Geldard supposed that there may have existed something inherent in this method that prevented the realization of the normal facilitation effect. Two experiments were reported both employing the critical frequency of flicker as the measure of sensitivity. In the first a foveal stimulus of constant brightness was used in connection with several intensities of peripheral stimulus, the critical frequency being determined for each of these conditions; in the other, the reverse conditions obtained, "with a constant flicker frequency the brightness of the foveal stimulus necessary to just extinguish flicker was determined for the same conditions of peripheral stimulation." Results showed that foveal sensitivity is dependent upon peripheral stimulation, the extent of the facilitation being determined by the intensity of the peripheral stimulus. The results by this method were judged to be sufficiently exact to support the original conclusion stated above. Discussion of the paper concerned details of the controls of stimulation.

Willie MacCook (Johns Hopkins University) reported results of experiments on the development of color discrimination in children. The *Ss* consisted of 110 children (45 girls and 65 boys) ranging in ages from 17 months to 6 years. After preliminary training tests, the children were required to differentiate by the matching method between (1) color of specimens of different hues with saturation and brightness controlled, and (2) between specimens of different saturation or brightness with hue controlled. In the first series the child was presented with four cards of different hues and a constant brightness and saturation value, and was asked to match successively a red, green, yellow, and blue card with the one like it in the four hues. Different degrees of brightness and saturation were employed in other similar experiments, each degree being constant in any one experiment. Finally at the end of the first series of experiments each child was asked to name the four hues involved. In the second series of experiments the child was presented with three red cards (then, in order, with green, yellow, and blue cards) of three different brightness-values and after that of three different saturation-values, the other of these two values being controlled each time, and he was required to find from these trios first a card to match one given him in the first degree of brightness, then in the second, and finally in the third degree. The same process was then repeated with saturation as the variable. Results were not given for separate ages, but the experimenter concluded from her complete results that (1) the abilities tested increased steadily, though not uniformly, with age throughout the range studied. This was true also for naming the colors. (2) Age differences in hue-discrimination were found, but no such differences were reliable in the case of brightness or saturation. (3) Matching ability appears 12 to 18 mo. earlier than naming ability. How much the discrimination ability in case of the hues is actually due to retinal development and how much to better technique or method at any age is obviously still an open question.

Beginning with the afternoon session, P. F. Finner (Florida State College for Women), in a general discussion of the recent trends in the explanation of the facts of social psychology, pointed out that certain terms formerly used—instinct, imitation, and sympathy—are being replaced by terms taking better note of the general social situations in which the individual grows up. For purposes

of social psychology, investigations in learning will have to show how different attitudes or sets and special trends in one's conduct are built up—how, for instance, one learns to become a paranoiac or an introvert, and how certain character or personality traits are acquired. These he was inclined to regard from the standpoint of originating in a social environment rather than being inherited as such. In the discussion following the paper, Finner maintained that leadership, *e.g.* can not be described as anything in the individual alone but in the social situation, and he suggested that emotions will eventually find their explanation also in the situation in which they occur. Mere names will no longer be accepted as explanations. Finner might have said that there is a strong tendency today in experimental psychology, as well as in sociology, to trace the sequence of antecedent conditions leading to any event, attitude, or ability. Moreover, psychologists and biologists have too many facts resulting from studies of individual differences to be inclined wholly away from suppositions of innate differences in structure.

John E. Winter (West Virginia University) compared effects of supervision of study on about 60 freshman college students at and below the 20th percentile of the class, with three control groups similarly selected. Both groups were in classes in 'methods of study' but the students in the experimental group were "put through the actual paces of study." The experimental group was reported as showing "decided superiority," But no satisfactory data on the reliability of the differences between this and the control groups were given. Moreover, there is no guarantee that study periods were equal. The students in the experimental group got at least as much study as they were put through! It is highly important to know whether 'methods of study' courses are sufficiently helpful to compensate for the time required for this additional course, and experiments on the problem should be seriously planned and carefully carried out under different situations.

J. F. Dashiell (University of North Carolina), discussing the objective character of 'legal intent,' pointed out a tendency in law to evaluate intent wholly on objective factors. Only the individual himself can really get at his intentions in any rational behavior, and there is considerable probability that in a number of cases intent is an afterthought even of the individual himself and not a true evaluation of the motives actually operating. In the case of negligence, for example, the law must consider it as dangerous conduct, or as conduct which may be harmful. In this case one's conscious experience may be almost wholly irrelevant. Intent is clearly the outcome of circumstances like degree of intelligence, training, and the influence of groups. If the individual denies harmful intent, there is no way of getting definitely at the facts except by means of his former behavior and other objective circumstances. Four marks of intentionality were mentioned: the anti-social character of the consequences of the act, the immediacy of these consequences, their high probability, and their predictability by the ordinary man. The view was expressed that to use these objective criteria the court might well profit by the aid of psychologists in supplying norms so that it may be known what the normal person under certain conditions would be likely to do. Such standardizations of conduct under practical conditions of life would seem to supplement in a valuable way the psychological tests now rather extensively used.

K. C. Garrison (North Carolina State College) reported an attempt to isolate the effect of intelligence, as an innate endowment, and of the socio-cultural factors upon the information-responses obtained from first-grade children. Intelligence

was defined as "performance on the standard intelligence tests," and it was admitted that no responses of this kind are due wholly to innate factors. In the experiments, mental age was determined by the Goodenough draw-a-man test in which language factors were not supposed to be prominent. Socio-cultural status was determined by a scale modified from various well known scales so as to be better suited to local conditions. The Sangreen Information Test for young children was used in getting information responses from the *Ss*. The *Ss* were paired as presently to be described, reducing the number originally chosen (160) to 105. Paired groups and information scores were as follows: (1) *Ss* with an average socio-cultural status score of 15.4 but with an average mental age of 82.8 months scored 136.7 on information; (2) *Ss* with an average socio-cultural status of 15.2 and with an average mental age of 59.8 months scored 124.1; (3) *Ss* with an average socio-cultural status score of 23.6 and a mental age of 73.4 months scored 141.1; and (4) *Ss* with a socio-cultural status of 6.7 and a mental age of 73.3 months scored 118.4. The average difference between groups 1 and 2 is thus 12.6, while that between groups 3 and 4 is 22.7. Thus greater differences in mental age give a smaller information difference where socio-cultural status is controlled than when these conditions are reversed so that the mental tests scored are controlled. Garrison therefore argued, both from data on pairing and from those on direct comparison of groups, that the socio-cultural status predicts information scores more accurately than mental age. Expressed in standard units, socio-culture was found to be 2.74 times as effective in information scores as was mental age. The correlation between socio-cultural status and information scores, moreover, was 0.43; and that between mental age and information scores, 0.24. These two combined by mutiple-correlation methods gave a coefficient of 0.54 with information scores. One is not surprised at these results in the prediction of information score. It would seem advisable to continue the investigation using, on the one hand, different sorts of socio-culture ratings and of intelligence tests, and, on the other hand, information tests which draw to various degrees upon facts not superficially observable. "Information "is evidently itself a complex thing, which should be considered in several of its meanings or implications.

J. M. Porter, Jr. (University of Virginia) analyzed the responses of white rats to multiple-cue problems. The animals were trained to follow a complex set of visual and tactual cues in a new type of circular discrimination apparatus. They were then given control runs in which only the visual or the tactual stimuli appeared, and also different control runs in which these two classes of stimuli appeared, and also different control runs in which these two classes of stimuli appeared in opposition to each other. It was found that "the stimuli as well as the modes of stimulation could be altered without the production of changes in behavior," also that the visual stimuli used were more effective controls of behavior than the tactual. It is probably not safe to generalize yet from these results.

At the business meeting on Saturday morning, H. M. Johnson was elected president for the ensuing year and Lyle H. Lanier was elected secretary-treasurer to succeed Paul L. Boynton whose term had expired; A. G. A. Balz and John A. McGeoch were elected to membership in the Council for three years and John E. Winter was elected to fill H. M. Johnson's uncompleted term. Joseph Peterson resigned from the chairmanship of the Committee on the Teaching of Psychology

because of lack of further time for this work and P. F. Finner, Florida State College for Women, was appointed to succeed him. The resignation of the Committee on the Teaching of Philosophy was also offered and accepted, it being understood that a new committee would be constituted. It was voted to accept a joint invitation from Tulane University and Section I of the American Association for the Advancement of Science, to hold the next meeting of the Society in New Orleans during the Christmas holidays.

Peabody College JOSEPH PETERSON

THE SIXTH ANNUAL MEETING OF THE MIDWESTERN PSYCHOLOGICAL ASSOCIATION

The sixth annual meeting of the Midwestern Psychological Association was held at the University of Chicago on Friday, May 8 and 9 under the presidency of Professor L. L. Thurstone. This association now contains a total of 324 members, 174 of whom are members or associate members of the American Psychological Association. There were six sectional meetings in which a total of 46 papers were presented and discussed. Nine members of the Committee of Fifteen on the first course in psychology met Friday morning. They discussed the work of the subcommittee on aims and in a preliminary way the reports of the sub-committees on methods and content of the course in elementary psychology. A number of informal round-tables were conducted on Friday evening.

The social events included a luncheon of the Psi Chi National Honorary fraternity on Friday noon, a tea, at which the wives of the department of psychology were hostesses, held on Friday afternoon, and the annual dinner on Saturday night attended by 150 members, at which Professor Carr was toastmaster and at the conclusion of which the presidential address by Dr. L. L. Thurstone was read. Under the topic, "The Measurement of Social Attitudes," he defended the point of view that social attitudes could be experimentally investigated by a method analogous to the method of impression in affective psychology. By enumerating a number of controls which were used to eliminate possible sources of error, he showed that his results were as reliable as many data obtained by the use of apparatus. A resolution, which was proposed during the business meeting, honoring the work of Professor Albert P. Weiss and expressing the sorrow of the members of the Association at his death, was adopted by a silent rising vote at this dinner.

The meetings opened with a section on physiological and sensory processes under the chairmanship of Dr. Franklin Fearing (Northwestern). The first paper in this session was read by A. Ford (Michigan) on "The Neutral Point in After-Images of Spectral Light." He found that positive after-images under the conditions of his experiment may last as long as 2 min. after stimulation ceased, whereupon the negative after-image appeared after an interval of from 3-5 sec. A white illumination of the background above a critical intensity during the life of the positive after-image reversed the positive image, however, into a negative one. D. B. Lindsley (Iowa) followed with a discussion of some possible neurophysiological sources of action-current frequencies. Preliminary results, obtained from the internal popliteal nerve and the gastrocnemius muscle of the rat during voluntary contraction and during the elicitation of the Achilles reflex, showed

that the frequency of action-currents from the motor discharge of the popliteal nerve is considerably higher than from the sensory discharge of this nerve. Action currents from the gastrocnemius muscle are of considerable higher frequency during voluntary activity than during reflex activity.

D. Thompson (Northwestern) described the relative accuracy of detection of emphasized words in spoken sentences by the sense of touch and by the lip-reader's vision. Her results indicated that emphasis is correctly located in over half of the cases, the accuracy increasing in order, from touch stimulation alone, lip-reading, to lip-touch-reading, thus corroborating Gault's data indicating that visual impressions of the speaker's face aid in the tactual discrimination based on the teletactor. Using the Weiss-Renshaw pursuit-meter, A. R. Lauer (Iowa State College) tested the reliability of certain types of pursuit reactions and the effect of induced optical defects. He found that the three different reaction patterns used were specific in nature and that, in general, induced myopia affected the performance very little, while induced hyperopia seemed to lower the efficiency of the modified Whipple aiming test.

R. Y. Walker and H. H. Jasper (Iowa) described the Iowa eye-movement camera. In this apparatus a single beam of light is reflected from the cornea of each eye, is magnified nine times, and is recorded on sensitized paper. This enables the experimenter to study the relative amount of coördination of the respective eye movements in reading. G. B. Paulsen (Ohio) read on factors influencing the 'coefficient of trait variability' of steadiness. The steadiness records of 80 *Os* obtained at 10 different sittings showed a marked practice effect from time to time although the level of performance remained practically constant during a sitting. This was explained in part by the failure to standardize the emotional state of the individuals from one sitting to the next. R. W. Husband and M. Brostrom (Wisconsin) discussed the influence of various types of music on body sway. Their tentative conclusions showed that music in all of its forms caused distinctly more swaying than standing in silence, jazz music causing more motion, with waltz tunes second in order. C. R. Griffith (Illinois) closed this session with a paper on "An Empirical Study of Choice." Using track men as observers he found that the most satisfactory 'take off' in the pole-vault, broad jump, and sprints gave evidence of complete muscular adjustment so that introspective analysis revealed sensations from no one muscular group as contrasted with another. The cues in other words were not always conscious.

Section B on Child Psychology was conducted by Dr. Herbert Woodrow (Illinois). In a paper read by K. C. Pratt (Ohio State) on the generalization and specificity of the plantar response in newborn infants, the author maintained the probability that any specificity which appeared can not very well be ascribed to the setting up of discriminatory responses and that analysis of the effector segments involved upon stimulation of different cutaneous areas of the foot and leg furnished a measure of the degree of localization or specificity in these regions. E. B. Greene (Michigan) discussed some growth curves of human structure and performance. The development and training of bladder control in infants was next discussed by H. F. Scoe (Iowa). The development of this function in 40 infants, age 3-18 mo., seemed to be that of a simple motor or mental process following the characteristics of a general curve of learning. It was found to be related, however, to such developmental processes as teething, walking, prehen-

sion, and physical ratings. C. R. Garvey (Yale) reported on the sleep activity of 22 nursery school children as recorded by the Johnson kinetograph technique over a total of 3339 nights. The form of the sleep curve was discovered to be essentially the same for all children as contrasted with the large individual differences found in adults. Since violet ray treatments tended to increase the quietness of sleep, it was suggested that the mid-winter decrease is a function of the poverty of ultra violet rays during that season. Some factors which affect instability and mood level in nursery school children were presented by M. A. M. Lee. The records were scored for shifts of mood and average tone. H. M. Williams (Iowa) discussed some aspects of the motor rhythmic performance of young children. By measuring the accuracy with which a subject tapped in synchronism with a mechanically sounded pattern at intervals of 0.5, 0.67, and 1.0 sec. between stimuli, the author found that the accuracy increased with chronological but not with mental age, and that a genuinely rhythmical or configurational type of response was obtained, especially at the two fastest rates.

In the afternoon there were again two simultaneous meetings, Section A on learning, with Dr. Hulsey Cason (Wisconsin) as chairman, and Section B on social and applied psychology, with Dr. Herbert Woodrow (Illinois) in the chair. In the former section H. N. Peters (Missouri) discussed the time factor in elimination of culs-de-sac in stylus maze learning with 30 college students as subjects. Using the Warden multiple-T maze he discovered that a consistent negative correlation existed between the time spent in culs-de-sac and the difficulty of elimination. He also discussed certain implications found in this and other conclusions respecting theories of learning. Conditioning as a function of the interval between the conditioned and the original stimulus was next reported by H. M. Wolfe (Ohio State). Her results indicated that backward conditioning is possible though very small in effect. The more practice that the subjects obtained, the less frequent became the conditioned responses. H. Gulliksen (Chicago) discussed the transfer of response in human subjects. His subjects, tested in two groups, were trained to react to visual symbols with and without punishment. He found that response to stimuli not previously presented was influenced by the similarity of the new stimuli to the familiar ones. J. B. Stroud (Kansas State Teachers College) discussed the effect of varying complexity of material upon the form of learning curves. T. C. Scott and B. B. Nelson (Ohio) compared the high relief finger-maze with an improved form of the stylus-maze indicating that learning on one type did not readily transfer to the other. Motivation versus repetition in learning was discussed by H. B. Reed (Fort Hays Kansas State College). By having his *O*s draw various designs under four different conditions, he found that practice without motivation was useless while an effective practice was proportional to the degree of accuracy in the knowledge of results. The section was concluded by a paper on "Learning Difficulty and Analogic Change in Two Types of Linguistic Structure" offered by D. L. Wolfle (Ohio State). Two sets of 16 nonsense figures were used to create an artificial language, one more systematic than the other. The learning curves for both languages were of the complete S-shaped type. For the homogeneous language the curve was symmetrical about its inflection point, while the curve for the heterogeneous language was assymmetrical. The conclusion was reached that the type of linguistic structure is a factor in the development of analogic change.

Section B opened with a paper by H. Woodrow on "Quotidian Variability." He investigated the index of an *O*'s daily or quotidian variation and suggested several uses of this index, such as the measurement of the adequacy of instructions in controlling the constancy of the subject in attitude; the measurement of quotidian stability of attitude in individuals; and the comparison of different traits as regards "trait variability." In an attempt to obtain a rating scale for psychologists, M. L. Reymert (Mooseheart Laboratory) computed a scale of five major categories by means of which a professional psychologist could be rated. The aims in the introductory course in psychology were discussed by A. R. Gilliland (Northwestern). His chart showed a summary of questionary returns obtained in connection with the special committee on the teaching of elementary psychology sponsored by the Midwestern and the Southern Associations. Among his results the tendency of universities toward scientific psychology and among colleges toward applied psychology stood our prominently. A 'realistic-energic' psychology was proposed by J. P. Porter (Ohio). By means of a chart he summarized current psychological theories and tried to present a compromise which would be both realistic and dynamic. Psychology was presented as a scientific study of cumulative changes and integrations of mental energy in adjustment to external situations and in self-development. What happens when the second judgment is recorded in a true-false test was answered by E. E. Lamson (Cincinnati). Only 2% of the first judgments were changed. She maintained that it was much safer to change a judgment from true to false than contrariwise. C. H. Marple and N. C. Meier (Iowa) reported on the comparative influence of group and expert opinion at various age levels. The influence of both group and expert opinion on answers obtained to controversial questions was considerably greater for the younger groups and declined fairly steadily with age. It was equal to about three times that of chance. A. D. Annis (Iowa) discussed the induction of opinion through social suggestion by means of 'planted content.' Ninety-eight percent of the subjects were favorably biased in that group, and 86% of the subjects in the unfavorable group were unfavorably biased. R. C. Peterson (Chicago) read a paper on the effect of motion pictures on the social attitudes of high school children. Statistically reliable changes in attitude have been found on such issues as war, nationality, and punishment of criminals before and after seeing a motion picture. T. W. Forbes (Ohio State) described a miniature highway apparatus for investigation of safety in automotive driving. The driver manipulates a miniature car along a path mounted on endless ribbons by means of the usual steering and acceleration mechanisms found in the standard automobile.

On Saturday morning two simultaneous sections were again provided. Section A on Animal Psychology was conducted by Dr. Max Meyer (Missouri). In this section P. T. Young (Illinois) reported on a new experiment upon relative food-preferences of the white rat. He found that foods can be arranged in a uniform series on the basis of low and high preference. Since 18 rats in two experimental groups were markedly similar in their preferences the order preferred may be characteristic of the species. O. C. Ingebritsen (Chicago) discussed maze learning following lesion of the cervical spine. Some of these lesions interrupted the principal known tracts, afferent and efferent, between cord and cortex. He found that the operated animals, in spite of more or less persistent motor difficulties, were equal to the normals in terms of number of trials, time of learning, and margin

of errors. G. M. Peterson gave a preliminary report on right- and left-handedness in the rat. After four rats had been found to be predominantly right-handed, two left-handed and one ambidextrous, they were subjected to an operation in the motor and somaesthetic regions of the contralateral hemisphere of the cerebral cortex. This lead to a transfer in handedness.

An electrophysiological exploration of the central nervous system of the rat was discussed by L. E. Travis (Iowa) and J. M. Dorsey (Michigan). For contralateral homologous fields synchronization and isomorphism were most marked in the motor fields. Definite kindred relationships, which however were less marked, were noted in the visual and common sensory fields. These relationships were most wanting in the compared auditory fields. In both the ipsolateral and the contralateral heterologous fields a distinct departure from the above tendencies was noted in that asynchronization and anisomorphism were the rule. Cerebral action currents in the dog was described by S. H. Bartley and F. T. Perkins (Kansas). All of the gyri on the left hemisphere were explored as well as the middle and anterior ectolateral, middle ectosylvian, and the anterior and posterior boundaries of the coronal on the right hemisphere. When the results are interpreted in terms of a field theory the similarity found between the tremor rate in skeletal muscle and the volley rate in the records from the cortex was made intelligible. H. Klüver (Behavior Research Fund) demonstrated by means of a motion picture film that the cebus monkey was able to use tools as well as, or better, than Köhler's apes.

Section B on Emotion and Aesthetics was conducted by Dr. Chester Darrow (Institute for Juvenile Research). B. B. Nelson and J. P. Portor (Ohio) opened the program with a paper on "Adequate Emotional Stimuli and the Galvanic Experiment." They compared the results obtained from verbal stimuli with those resulting from a prescribed typhoid innoculation which gave rise to fear, pain, or both. The data showed that the latter form of stimulation was much more adequate in the production of the desired emotion in 85% of the cases. C. Landis (New York Psychiatric Institute) discussed the measurement of emotion or emotionality. He concluded that no physiological techniques or psychological 'tests' have a safe indicator of emotionality. He proposed that such techniques and tests be accompanied by introspective analyses and by studying the manner in which the physiological conditions become involved through habit formation or social situations. A study of the relative memory value of pleasant and unpleasant words by R. Stagner (Wisconsin) disclosed that pleasant words used as stimuli tended to be recalled over unpleasant words and that words with indifferent affective tone occupied an intermediate position. H. Cason (Wisconsin) also presented a paper on the learning and retention of pleasant and unpleasant activities. His project had to do with the relative number and duration of pleasant and unpleasant situations in everyday life and the efficiency of retaining and reproducing these activities. L. Skinner (Iowa) in a paper on "Some Aspects of Artistic Piano Playing" described the apparatus and technique which reproduces photographically passages played on the piano. From the record it is possible to analyze the relative degree of intensity and duration used and the synchronization of chords as demonstrated by different eminent artists and the same artist from time to time. The next paper on "The Vibrato as Produced on String Instruments" was read by S. N. Reger (Iowa). By photographically recording phonographic

material, he was able to analyze the rate and extent of the vibrato cycle, the pitch-curve or form of the cycle, and the percent of the total playing time on these musical instruments. J. Tiffin (Iowa) discussed the technique for studying pitch and intensity in speech and song with preliminary statement of results. By means of a semi-portable apparatus which automatically recorded the pitch and relative intensity of sounds in music and speech, he discovered that the average pitch inflection in good voices is about 2.5 times as great as the average for the poor voices and that good voices utilized a significantly longer percentage of the total time in actual vocal cord phonation. H. M. Johnson (Mellon Institute) in a paper on bodily positions in normal sleep criticized the Freudian doctrine which alleges a desire on the part of the sleeper to return to the fetal state and *not* to be born. He maintained that the argument which is supposedly supported by observational data and logical principles is invalid on both counts. By means of stereopticon slides, C. A. Ruckmick (Iowa) outlined a program for the study of the facial expression of emotion. The material was presented under five categories: an analytical study of the normal face under emotional stress, emotional expressions in abnormal cases, a genetic study of emotional expression in children and the lower animals, racial differences in emotional expression, and the influence of bodily posture in the complete configuration.

The afternoon session concluded with an informal report under the chairmanship of Dr. Harvey A. Carr (Chicago) from various laboratories including Chicago, Ohio State, Wisconsin, College of the City of Detroit, Michigan, Illinois, and the Lewis Institute. A business meeting of the Association followed.

There was a general impression that the meetings had been very successful. They were generously attended and a large number of institutions from the wide area of the middle west were represented. A vote of thanks was extended to the University of Chicago and to Mrs. Thelma G. Thurstone, chairman of the committee on local arrangements, for making such excellent provisions for the meetings.

University of Iowa — CHRISTIAN A. RUCKMICK

THE POUGHKEEPSIE MEETING OF THE SOCIETY OF EXPERIMENTAL PSYCHOLOGISTS

The 1931 meeting of the Society of Experimental Psychologists was held at Vassar College, under the chairmanship of Professor M. F. Washburn, on April 2 and 3. Fifteen members were present, and they reported upon the experimental work in progress at the Columbia, Cornell, North Carolina, Peabody, Pennsylvania, Princeton, Smith, Stanford, and Yale laboratories. The 1932 meeting of the Society will be held on March 24-25 at Columbia University, under the chairmanship of Professor R. S. Woodworth. K. M. D.

HENDRIK ZWAARDEMAKER: 1857-1930

To the lasting sorrow of all his colleagues and friends Hendrik Zwaardemaker, Czn., passed away on the 19th of September, 1930.

Zwaardemaker was born on May 10, 1857 at Harlem in Holland and was in 1897 appointed Professor of Experimental Physiology at the University of Utrecht, where he taught this subject with great knowledge and lively enthusiasm till he reached the age limit in 1927. His death is a great loss to psychology and physiology, for he was one of the most original and active investigators in those departments of science. His personal characteristics endeared him to his col-

leagues, while his scientific qualities of acumen and most diligent devotion to science aroused general admiration.

It is impossible in a few lines to give an adequate account of his work, which was published in more than three hundred scientific papers and in several books.

His principle activity was in physiology, but in *Psychology in Autobiography* he considered himself as a physiologist with psychological aspirations. In this quality his scientific career became most fruitful to psychology. I intend to refer in this note especially to that part of his activity.

As a young research worker he was deeply interested in the analogies between the sense of olfaction and the sense of sight. This was the reason he studied the primary excitations of olfaction and the concomitant nervous processes. In this way the olfactometer and the olfactie were born. In possession of these means of investigation he was able to analyze the phenomena of compensation and over-compensation of odors and to formulate his mosaic theory of olfaction. His research work concerning the electrical charge of odorous nebulae allowed him to consider the olfactory sense as dependent on a mechanism of adsorption, which catches all molecules that lower the surface tension. The odorous molecules, taken up in this way by the lipoid substances of the olfactory cells, are able to display their olfacto-chemical action. Desiring to measure the *minimum perceptible* of olfaction on animals and man free from external irritation, he constructed an odor-proof room, just as he later on built a sound-proof room to avoid all sounds in determining the threshold values of hearing. By ultra-violet radiation he succeeded in transforming or destroying the odoriferous power of odors. The zero-method, applied to the compensation of odors, made it possible to represent two sensations of olfaction as two vectors. Zwaardemaker tried in this manner to arrive at a new classification of odorous substances.

In connection with his general work on the refractory period of reflexes, he became interested in the problem of olfactory fatigue. The phenomenon of bilateral fatigue, unilaterally produced, led him to the conclusion that olfactory fatigue has to be considered as a synaptic phenomenon.

Starting his medical career as a young physician he soon became famous as an otologist. During his whole life he contributed greatly to the development of otology, always guided by his natural inclination for that field. We owe to him the law of presbyacusis, fundamental work on the sounds of speech, an accurate determination of the thresholds in the scale of pitch, methods of examining the efficiency of sound by means of the improved Rayleigh disk, and a theory of hearing. He related the important data of otology to the data of other departments of sense; and he gathered all these facts together in his remarkable essays on *Energetics in Physiology*.

In those essays he studied the processes of self-regulation in man and in living tissues. By a series of most ingenious experiments he learned to influence the automatism of tissues by changing the available quantity of potassium. In his mind he combined the action of potassium with its radio-active property, a conception which was attractive and subsequently became very fruitful. He devoted the last ten years of his life to the building up, with his collaborators of a new science: bio-radioactivity.

It is a duty and a privilege to bring a grateful tribute to his lasting scientific memory and at the same time a witness of the affection he evoked in the hearts of all his friends and students.

University of Utrecht A. K. M. Noyons

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Brain Mechanisms and Intelligence; a Quantitative Study of Injuries to the Brain. By K. S. LASHLEY. Chicago, University of Chicago Press, 1929. Pp. xiv, 186, pl. 11.

For a number of years Lashley, a modern follower of Flourens and Goltz, has been interested in determining what effect destruction of various parts of the cerebral cortex produces upon the formation of various motor habits in the rat. He has tested the animal's ability to learn a simple problem-box or a light-discrimination-box and various forms of simple mazes both before and after the destruction of different areas of the cerebral cortex. In these experiments he has used hunger as the chief incentive. The number of trials and errors and the time required before the animal learned to solve the maze-problem without errors were used as criteria. His observations are therefore mainly of a statistical nature. The present volume is chiefly devoted to the discussion of results obtained with the mazes.

Lashley's chief results may be summarized as follows. Destruction of the cerebral cortex produced no significant effect upon the learning ability of the rat in the double platform box, for it required more than 50% destruction to produce significant retardation. The brightness discrimination box gave similar results. Lesions destroying up to 44% of the occipital cortex did not significantly interfere with the formation of brightness-discrimination. He concludes that the rat's capacity to learn this habit is unaffected by any injury to the occipital half of the cortex including the entire visual area of both sides.

The greater part of the work deals with results obtained by testing rats in mazes both before and after destruction of the cortex in various areas of the cerebrum. Although the maze-habit was much easier to learn, it was usually abolished by destruction of more than 15% of any region of the cortex. The situation in the maze is evidently more complex; but it involves no unfamiliar elements. Orientation in a stable environment is a constant phase of the animal's behavior. The most important features of the maze-habit are a generalization of direction from the specific turns of the maze and the development of some central organization by which a sense of general direction can be maintained in spite of great variations of posture and of specific direction in running. Lashley's results show that the rate of formation of the maze-habit is affected in proportion to the extent of injury of the cortex. The animals, however, are capable of continuous improvement through practice; although the tendency to stereotyped acts stops their progress at a level of efficiency less than that of normal rats.

Lesions of various cellular areas of the cortex produce the same results. It matters not what area of the cortex is removed, the results are the same; hence the loss or retardation cannot be attributed to the loss of any localizable sensory functions in the cortex. The size of the injury is important, the location is not. The analysis of the data on the maze-habit in relation to the various cellular

areas failed to reveal any constant dependence of the habit on any single cellular field, a dependence which might be expected if the habit had depended upon any single sensory function. Lashley believes that the possibilities that maze-learning and retention are the products of cerebral sensory defects are very slight. Some of his other experiments show that the results cannot be attributed to motor defects, sense privation or olfactory mechanisms.

Lashley concludes that none of the studies of learning or retention of the maze-habit after cerebral lesion has given the slightest indication that the maze-habit is made up of independent associational elements. He found it impossible to discover differences in sensory or motor processes which would correlate with the loci of injury or account for the retardation. The diversities of behavior, such as disorientation, tendencies to perseveration, and the like, correspond somewhat with the magnitude of the lesion, but not at all with the locus. They seem to represent diverse degrees of deterioration rather than specific defects. The effects of lesions in rats resemble more closely a general dementia than any of the more specialized defects, such as apraxia or cortical anesthesia, in man.

Lashley's guiding idea in these experiments has been to show that there is a general mass-action of the cerebral cortex: that various regions of the cortex have an equipotential function in the formation of the various motor habits; that one part of the cortex may take over the function of another after cerebral destruction; that functional differentiation is largely independent of both macroscopic and microscopic structural differentiation; that, therefore, there is no localization of function in the cerebral hemispheres; that the sensory areas have no special functional significance; and that the functions of the brain do not depend upon anatomically organized systems of fibers leading to the cerebrum or within the cerebrum itself. In the support of this thesis he has used the rat, a rodent whose cerebrum is not only very small but exceedingly generalized, an animal in the cerebrum of which no one has ever postulated an organization sufficiently differentiated to be characterized by the term functional localization in the sense that the term is used to denote specialized areas and transcortical functions in the human brain.

The entire work reveals only the retentive, global, equipotential and mass functions of the cerebrum. Other than these general factors it fails to reveal and, in fact, according to Lashley, the study tends to deny any specific brain-mechanisms in intelligence, the presence of which the title of the book might lead one to infer.

Lashley's conclusions can hardly be called revolutionary since they have been anticipated by the work of Flourens (1824) more than a hundred years ago. Flourens' conclusions were based upon the behavior of pigeons and many other small animals after the cerebrum had been removed in part or in whole. The experiments of Goltz (1881) on the cerebrum of dogs led to substantially the same conclusions. The complete integration of the motor mechanisms below the level of the cerebral cortex has long been recognized. Lashley, with his more refined maze-technique limited to the rat, has fared little better.

It is hard to see how the maze-method, limited as it is to the motor performance of an animal which is integrated at various nodal points below the cerebrum, can be expected to give any decisive information upon the specific integrative meehanisms at the cerebral level. The linkage of the cerebrum with the cerebellum

through the cerebral peduncles and pontal nuclei should not be ignored in making sweeping generalizations of cerebral functions. The cerebrum is not an organ independent either of the sensory thalamus or of the motor apparatus. Its motor linkage is not only through the pyramidal tracts but predominatly through the cerebral peduncles and through the cerebellum, whose global functions in general motivation may be readily conceded.

Cornell University JAMES W. PAPEZ

Modern Scientific Knowledge of Nature, Man, and Society. Edited by FREDERICK A. CLEVELAND with 14 Collaborators. New York, The Ronald Press Co., 1929. Pp. viii, 592.

The purpose of this volume, we are told in a foreword, is to present in thirty-one short chapters a vision of the world in which we live as seen by men of science. Both the college freshman or sophomore and the reading public require orientation that will enable an "individual to see the world as it is and map out his mental courses." This is attested by the fact that "millions of volumes" have been sold during the last ten years on general science, history, and philosophy. Some idea of the scope of this book will be gained from a mere statement of the main topics discussed in its four divisions. The first main division includes three introductory chapters dealing with the present day need for perspective, pre-scientific cosmologies, and science, education and civilization. The second division, labeled inanimate nature, gives an account of the starry heavens above and the rocky earth beneath us, with a smattering of the newer physics and chemistry. Transition to animate nature in Part III is made by way of colloid phenomena, since the life process is "a sequence of complicated colloid changes." Part IV deals with human nature and occupies the largest part of the work.

It will be of special interest to the psychologist to find out how his science is being ordered in the general scheme of things. He will be disappointed to find that from the chapters on psychology in this book one would never dream that psychology has been an experimental science since 1879 and that specific problems attacked in the laboratory by methods common to physics and chemistry form the backbone of his science. After perusing this book the non-psychological reader will be able to toy with such words as functionalist, structuralist, and behaviorist (but not, alas, configurationist!) even though he hasn't the faintest notion as to how the psychologist goes to work to solve his problems.

From the table of contents and the index of subjects one might be led to believe that this is a glorious work indeed. But let him look up a topic, or read through a chapter, and he will be sorely disappointed, for the book is so thin as to leave hardly any impression at all with the reader. We are told that we cannot all be physicists, psychologists, biologists, but we can learn to think as the men in these sciences do! What a hodge-podge and welter of knowledge that would entail. How naïve the belief on the part of the one who wrote this that all knowledge actually goes together, in the present state of ignorance in which most of the sciences find themselves regarding fundamentals. He who would think as the physicist thinks would have to spend years in the physical laboratories—and likewise for psychological thinking. It is precisely this matter of thinking in certain terms that cannot be inculcated in a work of this kind.

The question which arises in the reviewer's mind as he contemplates this volume and others designed for the same purpose is: Are they worth a student's time getting oriented as they will orient him? The answer of the scholar, one fears, will be that such attempts are hardly worth the paper they are printed on. Yet it seems as if something in the way of 'orientation' is necessary for the undergraduate and the public at large, even though there be no royal road to knowledge. What is the fundamental difficulty with the thousands of books flooding our markets and claiming to teach everything?

In this case the difficulty does not lie in the caliber of the contributors, some of whom, like Shapley, Giddings, and Newman, bear illustrious names in science. Nor can the fault be laid at the door of the editor who seems to have coördinated the works of the several authors in masterly fashion. The difficulty, it seems to the reviewer, lies in the magnitude of the task which is simply beyond the scope of one book, in the nature of the material, which cannot be diluted beyond a certain point without losing its essence, and in the fact that a compendium of all the sciences cannot take the place of living contact with the data and methods of any one of them. Nor is the reviewer convinced that a smattering of all knowledge will give a fairer or truer picture of the world in which we live than a real knowledge of a single science gained by the sweat of one's brow. Do we need another Tennyson or Huxley to tell us that if we knew enough about a piece of chalk or the flower in a crannied wall, we should know the heart of nature? The last way leads us ever deeper into the world about us through a penetration which may be more restricted but ultimately, I believe, more illuminating.

Bryn Mawr College HARRY HELSON

Förstående och Minne en Undersökning Rörande Vuxnas Studiemetoder. By J. HJALMER FORSBERG. Upsala, Arvid Svenson, 1928. Pp. viii, 248.

This work constitutes an investigation of methods of learning and recall under actual class room conditions. It is an extension and elaboration of a preliminary study reported as "Studier rörande inlärendet hos vuxna" (*Svenskt arkiv för pedagogik*, 1917), the two investigations extending over a period of fourteen years. The subjects were pupils of the public schools, university students, teachers, and instructors. The methods employed in learning and recalling various types of subject matter were determined by personal interviews and involved the use of questionaries. Although the majority of the *Ss* had had no training in the examination of their own learning habits, some were trained *Ss*. The source data are the reports of eighty pupils, students, and teachers, from six different groups.

Since many of his data are introspectively acquired, Forsberg raises the question of the reliability of introspection as a method of research. Reviewing Wundt, Bühler, Hammer, Müller, and Stern, he concludes that they agree on the following conclusions: (1) the intention to retrospect disturbs the activity introspected; (2) an immediate, spontaneous retrospection yields the best results; (3) many mental processes are difficult to introspect; (4) the ability to introspect increases with training; (5) failure to remember, and suggestion, are the most serious causes of error; (6) although introspective data are often faulty, and need to be submitted to objective verification, the fact remains that introspection provides the essential basis for investigation, and without it other methods are futile;

(7) in view of the presence of individual differences it is important to employ introspective data from several persons. The sixth provides the author's justification of his method. He prefers to call his technique retrospection, and he seeks to meet the condition stated in (7) by employing many subjects.

Analysis of the reports of the *Ss* emphasized in general two different procedures: in order to grasp the contents of the material at hand, large sections were read; in order to memorize, small sections. He found eight memorization aids: (1) marking the parts to be memorized; (2) reviewing, summarizing; (3) illustrating; (4) grasping as a whole; (5) forming associations; (6) putting into practice; (7) mechanical fixation (rote); and (8) mnemonic schemes. Under all conditions of study, with the exception of rote memory and the use of mnemonic schemes, understanding was found to be the vital factor. It may take either of two forms, comprehension or experience. In comprehension the subject matter under consideration is objectified and reconstructed in terms of one's own norms. In experience it is apprehended in terms of one's own personality. The former is an intellectual activity, the latter is emotional.

The author concludes with "practical advice for students," similar in tone to almost any 'Aids to Study' so common in our literature. There is a bibliography of 247 titles.

The author seems to have started with the purpose of showing the importance of understanding in learning and remembering under conditions of actual classroom study. How much this may have affected conclusions cannot be ascertained. It is unfortunate that only indirectly is there any indication of the relative effectiveness of the various methods employed in studying. This seems to be a serious defect in method. Even the conclusion that all these methods are effective since they produced results is hardly warranted.

Syracuse University ERNST THELIN

The Psychological Methods of Word-Association and Reaction-Time as Tests of Deception. By H. R. CROSLAND. University of Oregon Publications, Psychological Series, Vol. 1, 1929, No. 1. Pp. 104.

From the standpoint of thorough control of experimental conditions, technique and procedure, and elaborate and careful use of the data obtained, this is the best study yet made of the association method in the detection of guilt. There is an excellent critical summary of previous work and a chronologically arranged bibliography of one hundred and nineteen titles.

The main portion of the study reports the application of a modified technique of the word association method to the discovery of the guilty persons in six actual crime situations. In four the offense was stealing, in one forging, and in one cheating. There were in all forty-five *Ss*, divided into an experimentally determined group of thirty-four 'innocents,' and a group of eleven 'suspects' of whom nine were experimentally determined, one was very doubtfully determined experimentally, and another belongs in the group because of confession despite non-experimental determination. The experimental determinations concerning the eleven suspects were checked by confession, partial and incomplete in one case, and complete by six; and by evidence in three other cases, while one case may be described as doubtful.

A number of significant points in the technique should be mentioned. The eighty indifferent word stimuli were the same for all subjects and were presented with the significant words in a standardized order. The indifferent words were chosen with more care than has usually been the case, 88% being common nouns. An improvement could have been made by confining these words entirely to the common noun form. It would have been better also not to have the last word in the series a critical one, since a number of the author's criteria of guilt involved the use of reactions to post-critical words and this reduced by 5% the number of such words otherwise possible. In all, forty different criteria of guilt are proposed. Thirteen of these are 'quantitative' (*e.g.*, pure significant, pure non-significant, post-critical, semi-significant), and seventeen are 'qualitative' in character. Of these latter five relate to the significance of the word response, six to the repetition of the responses, one to repetition of the stimulus, two to failures of response and one to compensating or concealing responses, while the remaining two are mixed summary criteria. Eight other criteria refer to bodily changes—comparatively little use, however, seems to have been made of these. The remaining two are summary and comparative.

Overlooking, because of the lack of space, various evidences of subjective judgments and conclusions in the uses of the various criteria, one may say that the most pertinent criticism of the experiment from the standpoint of the author's obvious desire to show positive practical value of the method for the courts is perhaps his demonstration of its success merely in a very limited setting. He has shown that it is possible to pick objectively (with some doubt even as to objectivity) in cases where the suspect was definitely known to be in an experimental group; but before the association test, or any other psychological method of crime-detection, is offered for practical use in the courts, it should be demonstrated that from the study of data secured when the method is applied to an *individual* suspect there can be deduced evidence of his guilt or innocence which would be at least as reliable as the methods used in ordinary court procedure.

It is unfortunate that Crosland's colleagues did not, unknown to him, present a group of *Ss* with a purely hypothetical crime, concerning the details of which all of the group of suspects were ignorant. The results obtained under such conditions would probably have been most illuminating.

It should be added that this study includes the presentation of raw data for both reaction-times and reaction-words. There are also elaborate statistical studies of the data secured.

University of North Carolina HARRY W. CRANE

The Pueblo Potter. By RUTH BUNZEL. New York, Columbia University Press, 1929. Pp. 134, 38 plates.

This book represents a notable departure from the routine. In it the problem of creative imagination (viewed as a social and behavioristic process) is approached from a novel angle. Inquiry centers on the respective roles of group tradition and the urge to individual expression in the work of the primitive artist, the captions 'Stability in Decorative Style,' 'Symbolism,' and 'The Personal Element in Design,' accurately defining the major issues. First-hand contact with the problem was sought by way of apprenticeship in the making and decorating of pottery

in two of the pueblo villages of Arizona and New Mexico. For data bearing upon the sequence and mutation of styles of design, and the origin of local variants, the author had recourse to local and museum collections. Supplementary evidence was derived from ethnological and stratigraphic records, many of which tap a remote prehistoric past. A superb series of plates provides the reader with illustrative examples of household and ceremonial pottery, ancient and modern, from Hopi and Zuni pueblos.

The major contribution of the study lies on the borderline of the socio-psychological. In primitive no less than in civilized groups, the author demonstrates, individual artists, whose ability to originate new styles or adapt old approaches genius, make their appearance. While the effect of extraneous stimuli (migration, archaeological excavation, competitions incited by white influence) are duly weighed and examined, specialized individual endowment is stressed as the determining factor. Only in ceremonial pottery (characterized in general by crudity of workmanship and design) is the free inventiveness of the decorator restricted by a rigid and conventional symbolism. In her planning of the decorative schemes of household jars and bowls, the native potter relies on individual fancy, on visualization and on dreams. Of this Bunzel was able to secure signal evidence, though handicapped in her inquiries by the intense hostility and reticence of the pueblos toward the whites.

Due in part to the obstacles in the way of complete *rapport*, the narrative as a whole adds little to our knowledge of the imaginative synthesis. Perhaps the resolve to envisage art as a form of behavior works further disadvantage, for it entails the limitation of the 'aesthetic' to the formal problems of unity, thereby obscuring certain of the subtler implications of the material. The psychologist whose interests lie in this field will, nevertheless, find the volume an invaluable sourcebook. Three appendices (33 pp.) of unique worth reproduce, in black and white or in color, designs drawn freehand, from memory, by Hopi, Zuni and San Ildefonso potters. On the page opposite each reproduction, the artist's own account of the meaning of the patterns (composed for the most part of the familiar 'cloud-step,' scroll, feather and arrow-head motifs) is recorded; first as given at the time of drawing, and, secondly, after the lapse of a year.

Lack of uniformity in the two accounts, as well as in the interpretations assigned similar elements in different settings, is believed by Bunzel to refute the views of earlier workers, who held that a symbolic (or ritualistic) significance attaches to all such patterns. These discordant protocols furnish, however, by their very lack of fixity telling evidence of symbolism of a subtler sort, semi-metaphorical in texture. They hint, in fact, an emergent capacity for genuine aesthetic apprehension or *Einfühlung*. Precisely because of this flexibility in the elusive and shifting meanings of their components, such designs offer a freer scope to imaginative function, with comparatively little alloy of the practical or 'magical.'

If the published records are indeed authentic samplings, these household specimens of pueblo pottery are for their makers and users true artistic objects; not incomparable in effect to the 17-syllabled imagist poems (or Hokku) of the Japanese. The semi-formalized motifs of cloud and lightning, rain and vegetation, fused by the potter in these decorations are no longer either strictly ritualistic, nor yet merely realistic and representative. Long association with the nature-

magic of the pueblos has, however, charged them with characteristic feeling-moments—awe and dread, wish and desire, depression and elation. These, no less than her delight in contrast, rhythm, balance, and unity achieved in the face of many obstacles, must enter into and color the primitive artist's formal manipulation of the elements at her disposal. The phase of Bunzel's method fruitful of matter of this type—the raw material of aesthetics—merits further exploitation. One would like to see it pressed into service to decode the motivation of the modernistic decorative style of Acoma, with its forceful reversible-perspective patterns.

Though certain of the conclusions would seem to exceed the warrant of the scanty data available, the text as a whole merits careful scrutiny from psychologist and anthropologist alike. The zeal, insight, and discretion of the author in assembling her facts is beyond cavil; the discussion, well-organized and fluent. The volume a royal octavo of distinguished format—is issued under the editorship of Boas, representing, we are told, one segment of a more extended survey of peublo culture, under the auspices of the Southwest Society and the department of anthropology at Columbia. In addition to the plates above mentioned, a suggestive bibliography of related titles is appended. The inclusion of a map, indicating the location of the sites from which examples of decorative style are drawn, would immensely facilitate the general reader's grasp of the argument.

Cornell University ELSIE MURRAY

Social Psychology: The Psychology of Political Domination. By CARL MURCHISON. Worcester, Mass., Clark University Press, 1929. Pp. x, 210.

The aim of this book is to establish the thesis "that social psychology deals with those human characteristics that make political life inevitable." "It seems reasonable to assume that political life is the result of differences and conflicts;" and, therefore, the measurable differences between individuals are the fundamental data of a science concerned with the study of the domination of certain individuals over others. The method employed to establish this thesis is to assert the inadequacy and unreality of all other approaches to the science.

After the introduction, Part II takes up the discussion of "the fugitive nature of social behavior-patterns." The reality underlying all these fugitive patterns is the fact that individuals differ one from another and that by virtue of these differences certain individuals excel and dominate. The control over the labor of others is inevitable and is essentially the same thing, whether it is termed slavery, peonage, employment, or communism. The admirable life-achievements, victory, asceticism, poverty, knowledge, service, wealth, and success, are simply various roads to prestige, while the moral life-achievements are those which anyone can accomplish and which compensate the mediocre for their failure to achieve real distinction. Human rights such as liberty, freedom of speech and of religious worship, pursuit of happiness, and equality, are all illusions; and in the attempts to uphold them "all that has been achieved is trouble." In short, in spite of disguise, conflict and domination is the only real social behavior pattern.

Part III is a criticism of "some historical theories of political life." The views of Plato, the Christians, Machiavelli, Rousseau, Bodin, and Montesquieu are discussed, along with the theories of socialism, anarchy, and democracy. Of these,

only the theories of Bodin and Montesquieu "harmonize . . . with the measurable characteristics of human nature." Machiavelli was half right, since he recognized selfishness as the foundation stone of political life, but he failed to take into account the fact of individual differences in selfishness.

Part IV deals with "some persistent hypotheses of social psychology." The unreality of 'social forces,' 'instincts,' and 'the group mind' is asserted, the usual criticisms against them being given. In a brief but astonishing chapter the entire foundation of "Adult Human Psychology" is called into question, because that branch of psychology attempts to describe "the individual" when no such person exists in reality.

In Part V it is maintained that social behavior cannot be adequately treated in terms of 'instincts' or 'drives,' which names merely disguise the great variety of individual behavior patterns. "A description of the mere facts of inequality . . . is in itself all that is necessary for an adequate description of social phenomena." Radicalism and conservatism are merely points of view developed by persons of unequal economic status. Social cohesion and disintegration are illusions. Inequalities between individuals and societies are the realities. "Briefly, the phenomena of social disintegration are the observational delusions brought about by excluding the activities of the winning side from the field of observation."

Murchison's thesis emphasizes a group of important data that have been much overlooked in social psychology; namely, individual differences. But he seems to have fallen into the 'one track' fallacy which so besets the social sciences. Individual differences are not merely *important* data for him. They are the *only* data. There is just one fundamental relationship between individuals, and that is conflict leading to domination. There is just one 'real' way of looking at any complex set of phenomena. It is 'unreal' to view the history of the early centuries of the Christian era as depicting a disintegration of the Roman Empire; what "really" occurred was a struggle in which the barbarians won out. Why, may we ask, are not both points of view valid?

The wide and heterogeneous range of subjects covered in the book makes it necessary to deal with everything in a rather over-simplified manner. This fact, while it detracts from the value of the book as a critical contribution to scientific theory, might, as the author suggests, make it valuable for use in classes where "playing with ideas" is considered to be of greater importance than the imparting of information.

Colgate University C. W. YOUNG

Studies in the Comparative Abilities of Whites and Negroes. By JOSEPH PETERSON and LYLE H. LANIER. Mental Measurement Monographs, Serial No. 5. Baltimore, Williams & Wilkins Co., 1929. Pp. vi. 156.

In the introduction the authors remark that "a useful check upon the reliability of a given race difference obtained in any locality and under any specific set of circumstances is to take what seems to be fairly representative samplings from widely different environments and to compare the various results as checks upon one another with a view to determining just which factors persistently yield differences in favor of one or the other race." (p. 5). This type of approach seems to me to be of primary importance, and the results reported in this monograph undoubtedly represent a significant contribution to the subject. It is rather

disconcerting to find, therefore, that in spite of my complete agreement with the methodological point of view, these same results suggest one interpretation to the authors, and a totally different one to me.

The first part of the report is based on tests given to 12-yr.-old white and colored children in the public schools of Nashville, Chicago, and New York City. The tests used were the Binet Group test, the Yerkes Pre-adolescent Point Scale, the Myers Mental Measure, the International Group Mental Tests, and Peterson's three ingenuity tests,—the Rational Learning test, the Mental Maze test, and the Disk Transfer test. Not all of these tests were given to all of the groups, but on the basis of the inter- and intra-racial comparisons which they were able to make, the authors report that while there is a marked superiority of white over colored children in Nashville, as one goes from Nashville to Chicago to New York not only do the differences of race medians in intelligence scores decrease regularly, but in New York the negroes have actually surpassed the whites in one test (Rational Learning) and have practically equalled them in the other two, the Point Scale and the Myers picture tests (pp. 96 ff.) It would seem that when we take "samplings from widely different environments" we do not obtain persistent "differences in favor of one or the other race."

What do these results show? The authors rightly urge the need of "many cautions that the wise will observe in drawing conclusions" as to innate group or race differences (p. 43). They point out that there are three possible interpretations for the very excellent and rather unusual comparative showing of the negro group in New York: (1) the New York negroes are a highly selected group as compared with the American negro in general, (2) the environmental advantages from living in New York have raised the scores of the negroes there, or (3) the white sampling there may be inferior in ability to a truly representative one (p. 48). They are inclined to question this third possibility, because the New York whites did not make an inferior showing as compared with the Yerkes norms, and they surpassed the Nashville whites in the Rational Learning test. They add: "We cannot, from our results, decide in favor of any of these possibilities, and must await further data for a solution of the problem" (p. 48).

It is not quite clear to me what "further data" justify the authors in forsaking this cautious point of view for a fairly definite stand in favor of selection as the explanatory principle. They "assume that the tendency is for the negroes of the best intellectual ability to go into the Northern and Eastern states—an assumption which all intelligence-data support" (p. 96). But this assumption is just what remains to be proved; the intelligence-data show that the negroes in the North and East do better on the tests, but they do not indicate whether such superiority is due to differences in innate ability or to the superior educational advantages which they there enjoy. The authors regard the decreasing percentage of negroes in the total population as one goes from Nashville to Chicago to New York as indicating an increasing severity of selection, but as the *actual number* of negroes in the three cities decreases in the opposite direction (New York to Chicago to Nashville) it is difficult to see why the selective process should necessarily operate in the way the authors suggest. Other things being equal, the fact that there is such a large negro community in New York City, with a well-developed and practically self-sufficient economic and social life, might contribute to an easy, rather than a very severe type of selection. As the authors them-

selves point out in another part of the monograph, we need further data before we can reach a decision. That "there is apparently developing in New York, under the more severe struggle for existence a highly selected negro population which represents the best genes in the race" (p. 99) is at present an unproved assumption.

Some of the results which the authors themselves report seem to point much more directly to the influence of the New York City environment. Table 4 (p. 16), shows that on the average the native-born New York negro is in school grade almost a full school-year in advance of the negro born in the South (but who has also been "selected" to New York, be it noted!), half-a-year ahead of the negro born in the West Indies, and only about a quarter of a year behind the median grade norm for the city. Further the negro child born in the South but now living in New York is just about as much retarded as the Nashville negro child (p. 15)—who has not been "selected" to New York. There is a substantial correlation between school grade and years lived in New York (pp. 16 f.). The teachers in New York uniformly report that the children from the South are very poorly trained. The results with the Yerkes Point Scale in New York show a superiority (not quite reliable statistically) in favor of those subjects born in the North over those born in the South and the West Indies (Table 35, p. 92). All of these considerations, together with the known statistics of expenditures for negro education in the Northern and Southern states, seem to me to argue rather strongly—always admitting the need for caution—in favor of "environmental advantages" rather than "selection" as explaining the results.

It is evident that the problem of "racial differences" is still at the stage where different students, doing their best to free themselves from previous bias and to exercise that caution which the difficulties of the problem impose, can still arrive at quite different conclusions from the same results. It does seem to me, however, that the authors have put their fingers on what is perhaps the crucial point in the present status of the problem. We know that different samples of the same race do differ markedly, and the question to be decided is whether "selection" or "environment", or both, are responsible for the differences. It is the problem of selection which must now be tackled directly, and it may be that here the "crucial experiment" in racial psychology will have to be performed. This monograph makes a significant contribution in focussing attention so clearly upon this problem, and in bringing results to bear upon its solution, and it is to be hoped that the "really big project" (p. 5) which the authors regard as necessary in order to obtain definite results in this field will soon be forthcoming.

There are a number of other interesting results which would merit careful discussion if space permitted. The second part of the monograph reports the results of a study of white and negro adults (students of the State Teachers College at Murfreesboro, Tennessee, and at the Agricultural and Industrial Normal College at Nashville, respectively). There is marked superiority of whites over negroes in the Intelligence tests; reliable superiority in all the Seashore tests but Rhythm (in a former study the Fisk students surpassed the Peabody students by a reliable difference in the Consonance test); slight superiority in the mechanical abilities tests (Stenquist and others); the Downey will-temperament tests gave no significant results; there was no significant relation between lightness of skin color (in the case of negro subjects) and intelligence.

There is a bibliography of 66 titles.

Columbia University OTTO KLINEBERG

Creative Imagination: Studies in the Psychology of Literature. By JUNE E. DOWNEY. New York, Harcourt, Brace and Co. (The International Library of Psychology, Philosophy and Scientific Method), 1929. Pp. viii, 230.

Without claiming "any intention of contributing to aesthetic theory or to one of critical evaluation," Downey has "gathered up chips from the workshop" of laboratory investigation, in the hope of presenting "even an inadequate survey of the range and nature of the variational factor in the response of individuals to art" which will be of value to critic, teacher, and philosopher.

First, the author differentiates between those who possess the plastic imagination and those of the diffluent or emotional imagination. The plastic imagination constructs always in subservience to the dictates of objective reality, whereas the emotional imagination "transforms reality so that it may become carrier of all strange ecstasies and despairs of the inner life." At all events, desires and fears may give rise to imaginative creations, the organization of images yielding to the requirements of the situation, and not to the ideational type of the individual, for the conception of types has given way to a "recognition of more complicated situations." In imaginal situations individual differences manifest themselves in that some "project themselves into the midst of things, others keep on the outskirts as detached and critical observers." The effects of such differences are such as result from the objective attitude on the one hand, and the subjective on the other. In this connection, the author believes that the approach to imagery from the point of view of temperamental differences, as yet little explored, offers possibilities of great discoveries; for the indications are that the flexible image will be found among the artistic and seems to characterize the more imaginative individual.

Having thus treated fundamental differences in imagery, Downey passes to "The World of Words," and there suggests the significance of inner speech of the auditory, motor-auditory, visual-verbal, and vocal-motor varieties, although on this matter not much more is said beyond the suggestion of the influence of types on composition and reading. Under "The-Word-In-Itself," however, the author indicates the richness of the word as a "detached consciousness," for then it "has a tendency to blossom into all manner of images, feelings, impulses. It is a focus of associations, often of exceeding richness. It is haloed with "meaning." But the word is not a similar experience for all. There are the so-called "dictionary-minded" persons for whom words call up definitions; the "concrete-minded" who change words into definite images; and the persons for whom words are "things-in-themselves." In each instance the effects of word perception and the results in creativeness will be different.

From the "method of style"—that is, passing from the reader's mental content to that of the author he is reading—if widely employed, it would be possible, states the author, to draw conclusions relative to poet or prose writer, and thus to determine with some accuracy both the nature of the imagery employed by the creative artist and his method of composition. Furthermore, color symbolism and synaesthesis play their rôle, inasmuch as "an analogy which the average reader finds forced or unmeaning probably represents a peculiar but natural, rather than reflective, mode of thought for the poet."

Perhaps the author's viewpoint may be epitomized in the following quotation: "When listing and describing in somewhat static terms the psychic material of

which dreams are made—the concretely patterned image in its many modalities and the verbal transcript whether isolated words or flowing inner speech—I insisted not only upon the evanescence of imaginal content, its extraordinary complexity, and the possibility of its functioning in fragmentary and syncopated forms but also upon the fact that a description in static terms does violence to the dynamic, fugitive onrush of thought, its quicksilver-like darting from one point of stress to another." This onrush of thought, images, and their dartings are conditioned (in broadest sense) by emotional irradiation, by psychical disguise, by a "cartooning consciousness" which selects fragmentary aspects to the exclusion or subordination of associated aspects, and by a "metaphorical consciousness" which enlarges the field of attention, placing two or more objects or situations in juxtaposition, thereby enriching thought. Further, the more active consciousness of poets, the need for the resolution of conflict or of compensatory activity, more than ordinary sensitivity to the environment, the release of inhibitions, the projection of self into an imaginary scene, perhaps at the same time achieving scientific detachment, the use of hypnotic devices—these are the special equipment of the creative artist in literature which distinguish him from other individuals.

In this study the author has brought together the relatively meager psychological data available, and she has attempted to systematize them with reference to certain recognized psychological concepts. However, by far the greater portion of the evidence presented in the book is based upon the introspective reports of readers, not of creators. The adequacy or inadequacy of Downey's interpretations must, therefore, depend, in part at least, upon the similarity of mental processes of reader and author. Due, no doubt, to the fact that the field is relatively unexplored, one leaves the book without a clear conception of how images and imagination enter into patterned creations, although some other functions of images and imagination are indicated. However, the volume is stimulating psychologically and aesthetically.

Cornell University FRANK S. FREEMAN

Experiments with Handwriting. By ROBERT SAUDEK. New York, William Morrow & Co., 1929. Pp. lxiii, 394.

This book represents a serious and elaborate attempt to reduce graphology to a scientific or experimental basis. The author exhibits thorough familiarity with both the theories of graphologists and with the experiments on the writing movement made in the psychological laboratory. He adds to these two bodies of data a large number of observations of his own, and reports in an informal way a number of statistical studies which he has made to test his own theories. The discussion culminates in the description of a method of character analysis illustrated by several examples.

A considerable part of the book deals with methods of inferring from the written forms certain primary facts concerning the movement by which the forms were produced. For example, the author lays down rules by which the degree of automatism or maturity, the speed and the naturalness or authenticity of the writing act may be judged. He also indicates how one may determine whether the characteristics of the writing are due to the condition of the writer himself

or to the nature of the writing materials which he used. This part of the discussion is reasonably convincing, though the author has not established the majority of his conclusions by objective means.

Much of the discussion of the individual features of handwriting and their symptomatic significance consists of a minute and detailed analysis of the objective ways in which the writing of different persons differs. At certain points the significance of these differences as signs of differences in character is indicated; but many objective differences are described without assigning to them a particular significance. A separate chapter is devoted to the graphological signs of dishonesty. The author reports that he checked his method by comparing his diagnosis with the known facts concerning several groups of persons, and found almost complete agreement. The book concludes with a set of directions for the analysis of handwriting accompanied by a number of examples of such analysis.

In his attempt to make graphology 'experimental' rather than 'metaphysical' the author usually offers reasonable interpretations in place of the usual figurative and fanciful ones of the graphologists. One is left to take the greater part of the method of interpretation on the basis of its reasonableness, however, instead of experimental verification.

University of Chicago FRANK N. FREEMAN

The Trauma of Birth. By OTTO RANK. New York, Harcourt Brace & Co. 1929. Pp. xv, 224.

This volume constitutes a "first attempt to apply the psychoanalytic way of thinking, as such, to the comprehension of the whole development of mankind, even of the actual fact of becoming human." . . . "In attempting to reconstruct for the first time from analytic experiences the to all appearances purely physical birth trauma with its prodigious psychical consequences for the whole development of mankind, we are led to recognize in the birth trauma the ultimate biological basis of the psychical."

The main purpose of the work is to direct attention to the biologically based law of the form which determines the content. The neurotic is defined as the individual who becomes fixed in the birth trauma earlier than most people. Socrates "was the first who succeeded in intellectually overcoming the birth trauma, and thereby establishes his claim to be the forerunner of psychoanalysis." The author draws extensively upon myth, primitive and ancient religious rites, art, and philosophy for evidence with which to give his thesis full scope. Leading topics treated are infantile anxiety, sexual gratification, neurotic reproduction, symbolic adaptation, heroic compensation, religious sublimation, artistic idealization, and philosophic speculation.

This work represents Freudianism carried to the *n*th degree. It would be difficult to imagine a more extreme development of the doctrines. Along with material that would seem to be reasonably within the realm of plausibility goes a mass of interpretations that would recommend itself for serious consideration only to the extreme Freudian convert.

Hamilton College PAUL C. SQUIRES